

Challenge to Scientists

BRITAIN has recently been experiencing the strange happenings that seem to accompany Mr Uri Geller wherever he takes his one-man show. Knives and forks have been bent, broken watches restarted and feats of apparent telepathy performed. No generally accepted explanation is yet forthcoming.

Although the phenomenon of Mr Geller is new to Britain, he is well known both in Israel and the United States for his performances. It is important that he claims to be doing these strange things not as an illusionist using sleight of hand or deceptive material but as someone invested with a previously unappreciated power. He is prepared to back up this claim very convincingly by performing in such inauspicious surroundings as the back of a taxi in the company of the Science Correspondent of the *Sunday Times*. If he is an illusionist he runs the risk of being detected every time he shows off his skills, particularly when his performance goes on to videotape. Further, any stagehand knows how an illusionist works, but none has yet revealed Mr Geller's secrets.

It needs to be said, however, that not everyone is convinced that Mr Geller is other than a great illusionist and that there seems to be somewhat more scepticism in Israel and the United States than has yet developed in Britain. For a fairly cool assessment *Time* of March 12, 1973, should be read. Nevertheless he has clearly created a *prima facie* case for further investigation and it is to be hoped that the proposal by the *New Scientist* that he submit to examination by its panel will be taken up, even though he has already been examined extensively by a team at Stanford Research Institute.

What does all this mean for the scientist—and not necessarily only the scientist interested in psychic research? There are two distinct challenges to him.

The first is that analysis of this phenomenon must be absolutely neutral and above board. One of the remarkable things about this whole affair has been the way the public has asked that it be investigated by scientists. It has been common to hear phrases such as "I will not be happy until a panel of scientists have pronounced on it". In mediaeval times it was priests, later it was noblemen, in Victorian times businessmen and now scientists who are the arbiters of acceptability and correctness. It is not possible to believe that this situation will last much longer though it is difficult to decide who will eventually supplant the scientist in this role—trade unionist, professional footballer or television announcer. Nevertheless, while this duty still falls to the scientist he has a great responsibility to be utterly objective and entirely open. There have been some doubts cast on the Stanford investigation because of the lack of publication of the results apart from a report at a conference. On the other hand it is undesirable that Mr Geller should be used either by someone delighting in the occult or exceedingly sceptical of it.

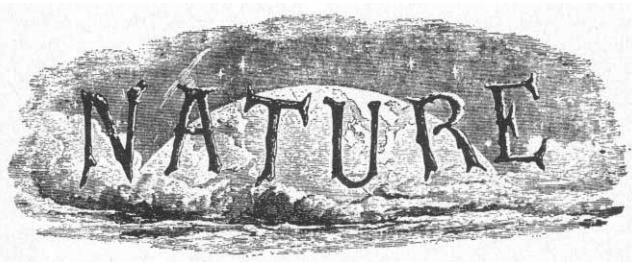
The second challenge to scientists will arise if investigations continue to turn up signs of psycho-kinetic powers, and with the present evidence this certainly cannot be ruled out. It would then be urgently necessary for the

scientific community to come to terms with something totally beyond its powers of explanation—indeed something which in a religious context would be called a miracle. Just as the public wants scientists to validate Mr Geller, it would also want them to explain him and, however awkward this question may be, it should not be avoided. If Mr Geller indeed possesses extraordinary abilities it is immaterial whether he is an isolated unrepeatable phenomenon or whether a large number of people can be taught the skills, and it is immaterial that he manifests the abilities in ways up to now better known to music-hall illusionists than to scientific investigators. The challenge would still exist—that well established scientific laws as apparent to laymen as to scientists are not inviolate under the influence of some presumed mental process.

It is difficult to see how research into the causes of such extraordinary happenings could proceed. One suspects that any approach which involved extensive instrumentation would end unsuccessfully. Technology has an unerring ability to suppress human skills. Nevertheless a boost for psychical research would be very welcome. There are too many loose ends lying around for comfort, and psychical research has not yet been able to shake off its mildly eccentric character and its ability to attract fierce criticism.

The viewing public, shown a chest operation under acupuncture one week and an exhibition of knife bending the next, is bound to ask searching questions about conventional scientific wisdom.

100 Years Ago



THE London Association of Correctors of the Press held a *conversazione* on Saturday last under the presidency of Mr. B. H. Cowper, editor of the *Queen*. We are glad to notice that the principal items of the programme were of a scientific character. Mr. E. R. Johnson, Chairman of the Association, read a paper on the past work of the Association, enumerating some of the papers and discussions on philological topics which had engaged its attention, and while commending the study of philology, the advantage of an acquaintance with one or other of the exact sciences was set forth. Mr. G. Chaloner, late Secretary of the Association, and lecturer on Chemistry at the Birkbeck Institution, enlightened the meeting as to some of the properties of hydrogen, accompanying his remarks with appropriate experiments. Mr. J. T. Young discoursed on the glacial period, and exhibited some fossils illustrative thereof. The wonders of the microscope and stereoscope also contributed to the enjoyment of the evening.

From *Nature*, 9, 114, December 11, 1873.



The Problem of Soviet Scientists

Professor John Ziman of the University of Bristol puts forward views on the need for scientific institutions not to acquiesce in any Soviet policy of repression of scientists.

MUCH of the concern amongst Western scientists about the situation of their Russian contemporaries arises from humanitarian, ideological or political motives. By our standards, life in the Soviet Union seems uncomfortable, inequitable, and constrained. But there are worse institutions in the world to protest against than the Soviet government; and by comparison with many of their fellow citizens Soviet scientists are relatively privileged. Andrei Sakharov and other political dissidents are brave men who deserve every sympathy; but the cause of freedom and humanity has many other heroes. The treatment of Jewish scientists and intellectuals seeking to emigrate to Israel is deplorable; but to some extent they bring this on themselves. It is a gross professional abuse of psychiatry to pretend that a prison is a mental hospital; but not so brutal as the police torture that disgraces many other states that pretend to adhere to the principles of the United Nations. In other words, although we may each, individually, give more or less weight to a variety of such factors, the state of Soviet science does not necessarily have the first claim on our humanitarian sympathies and moral indignation. In any case, action based on such motives does not lie within the competence of professional scientific organs such as learned societies and learned journals.

Soviet Science a Part of World Science

There remains, nevertheless, a central feature which makes this our business as scientists. Soviet science is not an independent national institution: it is an integral part of world science. Having made personal contact with our opposite numbers behind the Iron Curtain, we are obliged to help them when they are in trouble. Having shared in the pursuit of the same scientific goals, we cannot be indifferent to the disabilities under which their research is carried out. Since every scientist relies upon the skill and integrity of all the other scientists whose work he cites, it is a matter of the utmost concern to every non-Soviet scientist

that Soviet science should be of the highest quality: it would be very damaging to our own research if this significant fraction of the total world scientific effort were to fall into decay and disrepute.

From the Soviet point of view, also, much of their immense expenditure on science would be wasted if it were not up to the highest international standards. These can only be maintained by a vigorous internal climate of scientific criticism and open access to the world stock of scientific information. To achieve this, it is evidently in the Russian interest to draw in as many scientific conferences and scientific visitors as they can. But membership of this valuable club implies reciprocal courtesies and obligations. In return for such trouble as we may take in travelling to the Soviet Union, we may reasonably request that Soviet scientists participate fully in conferences in our own countries, and give freely of their scientific learning in visits to our own universities and laboratories. Severe restrictions on foreign travel by Soviet scientists are not only harmful to science in Russia: they are harmful and offensive to the whole international scientific community.

Even more serious is the evidence that attempts are being made by the Soviet authorities to violate the recognised norms of the scientific profession. It is essential to the intellectual integrity of science that due recognition be given for published work that makes a significant contribution to knowledge. Such high recognition as election to a national scientific academy is understood throughout the world as the reward for outstanding scientific achievement, measured according to the universal standards of international science. The Soviet Academy of Sciences is rightly regarded as one of the most distinguished bodies of this kind. Yet we hear of threats of expulsion against members of the Soviet Academy as 'punishment' for legal acts such as expressing dissident opinions or asking for permission to emigrate to Israel. If the Soviet Academy were to agree to act thus it would forfeit its position as a

leading institution of the world of science.

However much we may deplore it, we cannot insist that the sanction of dismissal from employment should not be used by Russian state organisations against scientists who are in political disfavour: membership of the Invisible Colleges of the international scientific community is not a guarantee of a well paid job for life. But further actions such as shutting dismissed scientists out of international conferences held in the Soviet Union, withdrawing their books from scientific libraries, and ordering that their previous published work should not be cited by other Russian scientists, are inadmissible. It must be emphasised that these vindictive procedures are being taken against Soviet citizens who have not been convicted of any criminal offence, but who have, on the contrary, earned their scientific reputation by hard honest labour over many years. The consequences of such a policy can only be the degradation of intellectual standards in Soviet science, and its further alienation from the world scientific community.

Scientific Standards Threatened?

These issues are so serious, and touch so deeply the whole practice and professional ethos of science, that we must not remain indifferent to them. The Soviet Union is not a banana republic making an insignificant contribution to science: it is a Super Power, whose brilliant scholars and immense laboratories play a major part in the advancement of knowledge. Not only the content, but also the style of scientific research throughout the world in the next few decades will owe a great deal to the way in which science is being done now in such great nations. It is evident that the Soviet government does not understand the delicate social structure of the Republic of Science, and persists in treating its scientific workers and scientific institutions as entirely subservient to immediate national political ends. Whilst insisting on the right to partake fully of the products of research, it is undermining the system that makes these products reliable and valuable. Unfortunately, this sort of short-sighted policy comes all too easily into the calculations of any power-hungry bureaucratic authority, whether nominally socialist or nominally capitalist. If we

acquiesce now in this particular subversion of the moral principles of the scientific profession, then in the long run we may bear the responsibility for the decadence of our own science at the hands of politicians, administrators, and other gentle experts in the arts of expediency.

Much can be done by individual protests and boycotts. But the responsibility for the protection of the norms of science rests squarely on the great learned societies, led in Britain by the Royal Society. If they fail in this duty, then who else could be blamed for inaction? What has prevented the Royal Society from speaking up in defence of the scientific ethos which it is pledged to preserve?

On the one hand, there are a few leading British scientists who hold that any criticism of Russian scientific policy is a political act motivated by disreputable ideological ends. On the other hand, there are administrative 'realists' who have no illusions about the disabilities of Soviet scientists, but who insist that more can be achieved by secret diplomacy than by open comment. These two extreme parties combine to prevent the whole subject from being properly discussed. For opposite reasons they express themselves strongly against any action, and thus paralyse the large middle body who feel uneasy about the situation but who have no great personal interest in upsetting the *status quo* of acquiescence. It is to the conscience of this group that my argument is addressed.

The most important and decisive action would be public recognition by

the Council of the Royal Society of the main facts concerning the state of science in the Soviet Union, and an expression of support for the basic principles of academic freedom and integrity. Whatever might be the overt reaction of the administrative organs of the Soviet Academy of Sciences, such a statement would make it quite clear to the main body of Russian scientists that we understand their present position and sympathise with their personal endeavours to contribute honestly to the world stock of human knowledge. In the absence of such a public statement, claims of influence by secret bargaining are without weight; the very fact that various officers of the Royal Society deal directly with the officials of the Soviet Academy can only suggest to other Russian scientists that they are ignorant of, acquiesce in, or even approve the corruptions with which we are here concerned.

Exchanges of Scientists

It is often suggested, both by Soviet apologists and by official 'realists', that the channels of communication must be kept open. They point, for example, to the Scientific Exchange agreements by which visits are arranged between the Soviet Union and other countries. In all honesty, however, it must be admitted that these exchanges are of such insignificant proportions that their value is negligible. The number of Russian scientists that visit Britain under such schemes is probably about the same as the number of Israeli or Australian scientists who come and talk with their

British colleagues. The truth is, also, that many of the present Russian visitors to the West are of very low scientific standing, and are apparently more competent at picking up snippets of information than communicating at the highest intellectual level with our own leading scholars. The average Soviet scientist of good repute has so little expectation of making a foreign visit himself that he would not be greatly perturbed if those particular exchanges were inhibited!

In any case, the flow along these channels is controlled almost entirely by the Soviet authorities themselves. It is not suggested that the Royal Society, the British Council, or other agencies should act in any other way than to welcome exchange visits by *bona fide* scientists. If Soviet officialdom were to react to criticism of their domestic policies by refusing to collaborate in such exchanges, the responsibility would be theirs. What sort of a bargain is being struck when one promises silence in return for an agreement that mainly benefits the other party?

And that is the heart of the matter. The present efforts of the Soviet government in the scientific and intellectual sphere seem directed with pathological intensity towards suppressing all internal criticism. For all the talk of *détente*—an issue which lies of course in the political sphere and therefore outside the competence of our scientific institutions—it would be tragic folly if we also were coaxed into silence about the injustices being done to our scientific colleagues and friends in that great country.

NEWS

Oil from Shale: Answer to the Energy Crisis?

by our Washington Correspondent

BENEATH a remote region of the Rocky Mountains lies a band of rock which contains enough shale oil to dwarf all the other United States domestic reserves of petroleum put together. Some 1,800 billion (US) barrels of oil are locked into the rock there, and up to a third of it is in deposits rich enough to be exploited commercially. Last week, the federal government took the first tentative step towards developing the resource, for Rogers C. B. Morton, Secretary of the Interior, announced that six tracts of federal land will be leased to oil companies next year so that they can begin producing shale oil.

Unfortunately, however, the sheer magnitude of the deposits has led to some unfounded optimism and grossly overstated predictions about the potential impact of shale oil production on fuel supplies in the United States. There are many problems ahead for those intent on developing the resource and there are factors which are likely to set physical limits on the rate at which shale oil will be produced. Thus shale oil is unlikely to provide salvation from the energy crisis, as some have predicted. But one thing is certain: the emergence of a shale oil industry will radically alter a large area of Colorado, Wyoming and Utah, and it will cause massive destruction of the environment.

The leasings announced last week will

allow a prototype operation to get under way and the objective is, on the one hand, to enable the oil companies to test the technology for producing oil from shale on a moderately large scale and, on the other, to enable the Department of the Interior to take stock of the environmental damage that results. After three or four years, the companies should be able to decide whether oil shale development is commercially attractive and the federal government will be able to decide whether the environmental costs justify throwing the whole area open for development.

The six tracts to be leased next year—two each in Colorado, Utah and Wyoming—cover a combined area of some 30,000 acres; the entire oil shale

field spans some 25,000 square miles. That is an area equal in size to the state of West Virginia or three times the size of Wales. Most of the land is owned by the federal government, which is thus in a position to dictate the pace and scope of development.

It has been known for a long time that huge quantities of shale oil lie under part of the Rockies and, in fact, some oil was produced from shale deposits before natural petroleum was discovered in 1859. But the cost of extracting oil from the shale has always made it commercially unattractive—at least until the energy crisis caused the costs of other energy supplies to increase. And the Arab oil producers have now turned out to be the best friend that potential oil shale producers could possibly have, for their embargo on oil exports to the United States has turned the shale fields into a goldmine almost overnight.

Shale oil is locked into rock in the form of a solid hydrocarbon called kerogen. To get it out requires the rock to be heated to about 900° F and the gases and petroleum vapours that boil off collected. Potential oil shale producers have been experimenting for several years with two different approaches to that deceptively simple task. One involves mining the shale, crushing it and heating it in a retort. The other involves either blasting or excavating a cavern beneath the surface, burning gases in the cavern to cause oil to flow out of the surrounding rocks and pumping the products to the surface. The former approach has received the most attention and will probably be the one adopted by most of the companies that will be bidding for the leases next year.

This is how the process will work. Oil shale will be mined either by open-pit mining where it is close to the surface—only a small area of the Colorado shale fields will be surface mined—or by the simple room-and-pillar method. It will then be crushed into marble-sized pieces and fed into a retort which will initially be capable of processing up to 10,000 tons of shale a day. Several different designs of retort have been patented, and they involve heating the shale either by burning some of the oil in the rock or burning some of the gases produced from the shale. One process also involves heating ceramic balls in a separate plant and using them to retort the shale. In any case, the resulting product is a viscous, low gravity, moderate-sulphur and high-nitrogen oil.

Once the oil and gases have been extracted from the shale, the problem is then what to do with the vast quantities of rock left over. The crushing and retorting process expands the rock considerably, so it cannot be simply fed back into the mines, and the only solution will be to dump it into canyons and to build up new slopes which will then

have to be revegetated. The oil shale region will thus experience something of a transformation.

The Department of the Interior reckons that the prototype operations will eventually lead to the production of some 250,000 barrels of shale oil a day and that, if full commercial development of the shale fields proves to be acceptable, 1 million barrels of oil could be produced by 1985. A simple calculation shows, however, that such a programme would be a massive undertaking, which, for a variety of reasons, may be close to the limit of production capabilities.

The sheer scale of such an operation is one stumbling block. It is reckoned that to be commercially attractive to exploit, the shale will have to contain at least 25 gallons of oil per ton of rock. Thus, for every barrel of oil produced (one barrel is 42 US gallons) two tons of rock will have to be processed and dumped. It follows that production of 1 million barrels of shale oil a day will require the mining, processing and dumping of 2 million tons of rock, and that would require a mining operation equivalent in size to the entire United States domestic coal industry. There will undoubtedly be a large increase in coal production in the next decade, so oil shale producers will be hard pressed to find the mining capacity to support a large industry.

Another, and ultimately more intractable, problem is the shortage of water in the area, for the oil shale fields lie in a semi-arid region. Water would be required in the retorting process—although one technology under study would require very little—and since an oil shale industry will lead to large urban development, there would also be increased demand for municipal water supplies. Thus, according to an environmental impact statement published in August by the Department of the Interior, “development beyond the 1-million-barrel-per-day level is possible, but at some point water availability could place a limitation on the ultimate size (of production)”. A similar conclusion is reached in a study to be published in a few weeks time by the House Committee on Science and Astronautics. Thus, mining and retorting of oil shale is likely to be limited for the foreseeable future to about 1 million barrels a day which, although significant, represents only about 5% of projected demand for petroleum in 1985.

Chiefly for these reasons, a good deal of attention is now being paid to the second technology for shale oil production, the so-called *in situ* method. If it can be perfected, *in situ* production would have the advantage that little or no rock would have to be brought to the surface and eventually disposed of, so many of the environmental problems

would not arise, and it would also use less water. The technology is not as well developed as surface processing technology, however, and according to the Department of the Interior, there are still some problems to be overcome. Nevertheless, the federal government is prepared to put more money into *in situ* research, for a five-year energy research and development plan being put together by the Atomic Energy Commission recommends that \$126 million be spent on such research by 1980.

Occidental Petroleum claims, however, to have perfected an *in situ* process which is now capable of commercial application. According to an interview with Dr Armand Hammer, the controversial chairman of Occidental, which was published in the *London Times* on November 24, the process involves blasting a chamber into the underground shale formation, injecting natural gas into it and burning the gas. Oil runs out of the shale, into the chamber, and can be pumped to the surface. Hammer made the astonishing claim that the costs of the operation are only \$1 per barrel, and he said that with massive capital investment the process could be producing large quantities of shale oil within three to five years.

Sources in the oil industry and in the Department of the Interior were sceptical of the claim last week, however, and an official of Occidental even said that he believes the claim that oil could be produced for \$1 per barrel “may have been misinterpreted”. The Department of the Interior also said in its environmental impact statement that “it is obvious that considerable further improvements in *in situ* recovery are still required before industrial scale *in situ* recovery could become a reality”. The impact statement also suggested that a major uncertainty in the *in situ* method is what happens to subsurface movements of liquids and gases, both before and after the retorting process. There is a possibility that toxic contaminants may find their way through the groundwaters into the Colorado River, which flows through the oil shale region and provides a lifeline for several million people who live downstream.

The Occidental process has, nevertheless, caused something of a stir among shale oil developers and *in situ* production will be closely investigated in the next few years. In fact, the two prototype tracts to be leased next year in Wyoming are particularly suitable for *in situ* production.

The vast oil shale deposits in the United States are thus likely to make a significant contribution to oil supplies sometime in the future, but they are not likely either to affect the relatively short term situation or to provide final salvation.

NUCLEAR POWER

Expected Defence

THE government's reply last week to the report of the Select Committee on Science and Technology on nuclear power chiefly defended the formation of the National Nuclear Corporation (NNC) in its current form (Cmnd 5499, HMSO, 1973).

The select committee had recommended that the government should take at least a 30% share in the new company, the remainder of the shares being open for tender. Any special management that the new company required should be provided on a contract basis, according to the select committee.

In the event, the government allowed GEC to acquire 50% of the shares and took up a mere 15% for itself through the United Kingdom Atomic Energy Authority (UKAEA).

The government says in its reply that "the basic need is for a company with the commercial, financial and managerial strength to meet the United Kingdom's requirements for nuclear power safely and to forecast cost, time of completion and performance. A company with these capabilities should also be in a position to compete successfully in the world market and would ensure that our substantial public investment in nuclear technology is exploited to the full and is suitably remunerated".

For this to be achieved, the government argues, "a unified management under the supervision of a principal shareholder who has a substantial financial commitment to the company and experience of the industry" is a main requirement.

The committee has also been worried by the prospect that GEC might withdraw from the new company. The government considers this unlikely, because GEC's financial commitment is "a serious and substantial one". Furthermore, the company has agreed not to sell its shares for five years without the consent of the government. If, later, GEC does want to sell, the UKAEA is to have first option on the shares.

The government also sets out to reassure the committee about the constitution of the Nuclear Power Advisory Board (NPAB). The committee said that unless the board was given an effective role it would be better not to appoint it. The government replies to this by stating that it sees the role of the board as being one of the greatest importance.

Equally the government defends the appointment of Lord Aldington, Chairman of the National Nuclear Corporation, to the board. The committee had argued that the construction company

should not be represented. The government says that it carefully considered the committee's comments, but believes that "NNC's views on all aspects of nuclear power development should be available to the NPAB and that this can best be achieved by Lord Aldington's membership. NNC's commercial judgment will be a very important factor in determining the direction and success of this country's future exploitation of nuclear power".

The government also declares that it is considering the committee's recommendation that the Chief Inspector of the Nuclear Installations Inspectorate should not have any other responsibilities.

The committee learned when it took evidence from Mr E. C. Williams, the Chief Inspector, that he is also the head of the Energy Technology Division within the Department of Trade and Industry. Fearing a clash of interest between the two jobs, the committee came out in favour of keeping them separate.

The government stated that it will take this into account "during further consideration of the arrangements that will be proposed for a Safety and Health Commission".

AIR POLLUTION

Emission Remission

by our Washington Correspondent

ALTHOUGH motor manufacturers have so far failed in their attempts to keep exhaust catalysts off all cars sold in the United States next year, they may yet win a partial reprieve from the full force of the Clean Air Act. Last week the Senate Public Works Committee decided to recommend to Congress that the timetable for meeting the strict exhaust emission controls specified by the act be relaxed a little. The committee will recommend, in short, that the controls be applied in California next year, as planned, but that they should not be extended to the other forty-nine states until 1976. This will be a year later than planned.

If the Senate and the House of Representatives take up the committee's recommendation, it means that exhaust catalysts would have to be fitted to all cars sold in California next year, since there is not enough time to develop other technologies for meeting the standards. But they would not have to be used elsewhere in the nation until 1976.

The committee's partial retreat will not, however, please the Ford Motor Company or Chrysler, since officials from those two manufacturers argued

COPYRIGHT

Carry on Copying

by our Washington Correspondent

A SPECIAL appeals court in Washington DC ruled last week that the copyright laws do not prevent libraries from photocopying journal articles or pages from books. The decision, which overturns an earlier ruling by a judge of the United States Court of Claims, has been warmly applauded by scientists since it means that libraries can continue to supply them with photocopies of journal articles for research purposes. But it has been roundly condemned by publishers who claim that it will drive yet another nail into the coffin of struggling magazines.

The court also said that it is up to Congress to place limits on the amount and type of photocopying that should be allowed, so the ball is now firmly in the court of Senator John McClellan's Subcommittee on Patents, Copyrights and Trademarks, which is considering amendments to the 1909 Copyright Act (see *Nature*, **244**, 535; 1973).

The ruling came in a suit filed against the National Library of Medicine and the National Institutes of Health (NIH)

at public hearings early in November that Congress should not force the use of catalysts at all; instead, they contended, more time should be given for the motor companies to develop better technologies for cleaning up emissions from automobiles (see *Nature*, **246**, 182; 1973).

Senator Jennings Randolph, chairman of the Public Works Committee, also announced last week that his committee is planning to hold more public hearings on a related matter—whether or not the control on emissions of oxides of nitrogen should be relaxed. The Environmental Protection Agency announced earlier this year that the original measurements of city air pollution, which led to the standards, were incorrect and that oxides of nitrogen may not be a great problem. Those hearings will be held in December or January.

It is unlikely that the Senate will adopt the committee's recommendation for relaxing the timetable specified by the Clean Air Act until it has also made a recommendation on the nitrogen oxides question. Thus, even if Ford and Chrysler manage to persuade other members of Congress that the committee's recommendations do not go far enough, it seems unlikely that Congress would be able to act in time to prevent catalysts from being installed on California cars next year.

by Williams and Wilkins Co., a publisher of some thirty-seven medical journals, including the *Journal of Immunology and Medicine*. The company alleged that the libraries had been infringing copyright laws by widespread photocopying of articles for NIH scientists and for other libraries. In 1970, for example, NIH libraries photocopied 85,744 articles (about 930,000 pages) and in 1968 the National Library of Medicine photocopied some 120,000 articles, mostly for smaller libraries which do not have subscriptions to the journals concerned.

Other publishing companies joined in the suit on Williams and Wilkins's behalf, arguing that the advent of fast copying techniques and the widespread and growing practice of passing photocopied material from one library to another is cutting into the sales of journals and will eventually force small circulation magazines out of the market. The full Court of Claims—essentially an appeals court—held last week, however, that if such photocopying is banned, medical research would suffer. But the decision was a vexed one, for the court was split four to three and one of the judges declared in a dissenting opinion that “the decision will be read that a copyright holder has no rights that a library is bound to respect”. Attorneys for the publishing company have indicated that they will probably appeal to the United States Supreme Court.

In the meantime, McClellan's subcommittee is considering a bill which would allow libraries to photocopy copyright works only if they have satisfied themselves that the material cannot be obtained through the usual commercial channels. In other words, no library would be able to photocopy a scientific article for a client, without paying a royalty to the publisher, if the journal is still in print. That provision caused such a hue and cry from library organisations, however, that McClellan reopened hearings on the matter in August; his subcommittee has yet to take further action.

ENERGY

Unnatural Gas

NORTH Sea gas will not last for ever. When reserves are exhausted, which is estimated to be in about twenty years time, what will there be to replace it?

One possible candidate now being actively studied is a substitute natural gas (SNG) made from coal. A plant for producing SNG is now being tested at Westfield, Fife. This is the first time that a gas with the calorific value of natural gas, about 800 British Thermal Units (BTU) per cubic foot, has been produced from coal on a commercial

scale. The burning characteristics of the gas are also similar to those of North Sea gas and it can be used in equipment designed for North Sea gas without any expensive modification.

The Westfield plant manufactures SNG in two distinct stages. First, the well established Lurgi process is used to gasify the coal almost completely and a gas of low calorific value, or ‘lean’ gas, is produced. This lean gas is then enriched catalytically to give SNG. It is the catalytic enrichment (a methanation) which is the key to the production of SNG and the Gas Board is loath to reveal the full details of its operation.

The Lurgi process converts almost all the coal into gas, unlike the system used to manufacture town gas which produces coke and tar as well. The lean gas so produced provides about 176 useful therms (1 therm = 10^5 BTU) from each ton of coal used but this depends on the quality of the coal. Town gas, on the other hand, produces only 75 therms a ton even though it has a calorific value of 500 BTU compared to the 300 BTU of the lean gas. The methanation process consumes energy, of course, but 145 useful therms of SNG gas can be obtained from each ton of coal.

This demonstration plant has not, however, been built with sales in Britain in mind. It is being financed, at a cost of \$6 million, by a consortium of about twenty North American companies and is being run by a subsidiary of the Continental Oil Company in association with Scottish Gas. The Gas Board has access to the results of the project—the process design was carried out by British Gas and Continental Oil

experts—but the board regards SNG as an insurance against the future and not the solution to an immediate need. The potential importance of the project is at present centred firmly in North America.

Fourteen Lurgi plants have been built around the world but only two are still in operation: the Westfield installation and another at Cecilburg in South Africa. Successful in their day, the other plants were made obsolete when plentiful supplies of natural gas became available. The Cecilburg Lurgi, a much larger undertaking, owes its survival to strategic considerations and cheap local coal.

In the United States, the demand for natural gas is increasing rapidly and, according to some sources, the reserves will only last for another ten to twelve years. Two plants are already in operation in North America based on another catalytic process developed in Britain to obtain SNG from oil. Nine similar plants are under construction. These plants are very successful but their future is now uncertain because of the current oil shortage.

The economics of the operation at Westfield depend on the price of coal and it is hoped that by experimenting with various slagging techniques (deliberately allowing certain concentrations of slag to build up in the furnace) the cheaper, low grade coals may be used. The United States has enormous coal reserves and if the Westfield project is successful it will provide a missing link from the chain of processes necessary to convert some of these reserves into SNG. The possible application of the process in Britain is, of course, further in the future.

Short Notes

Blowing in the Wind

A NOVEL aspect of the energy crisis was revealed in *Pravda* last week (November 27). Ten million tonnes of Soviet coal are, it seems, lost annually in transit. With the increasing mechanisation of the mining industry, the proportion of the fine fraction in each coal shipment has increased and faster freight shipment means that this fine fraction is blown away from open wagons, causing a loss of up to 15 to 20% of each shipment. This costs some 5 million roubles a year in counter-pollution measures.

The Soviet Committee of National Control is considering methods to prevent this loss. The most promising would seem to be that of Professor V. Beshkov, who suggests covering the shipments with a protective synthetic film, made from waste products of the cellulose and petroleum industries. Other proposals include mechanical

compaction of the slack, coking of the coal at the pit head to eliminate transport (the losses of coke in transit are considerably lower), and such elementary precautions as sealing up cracks in the wagons to avoid coal falling on to the track.

But why not buy a few tarpaulins?

Sliding into Europe

BBC-2 is running a series of programmes under the overall title *Life in the Nine* to explain how Britain may be affected by joining the European Economic Community (EEC). The programme on November 26 dealt particularly with EEC regulations applying to road transport. In the course of it Mr Geoffrey Cave-Wood, a member of the Road Hauliers Association, said that an increase in the axle and total weights of lorries from the present British limits to the expected EEC limits would be a good thing for many reasons, one of which is that the extra weights of these vehicles would enable them to stop more effectively.

NEWS AND VIEWS

A Better Future for ISC?

FOUR years ago there was speculation in *Nature* (222, 1216; 1969) on the future of the International Seismological Centre in Edinburgh. The ISC, it was suggested, should go into the seismogram storage and distribution business and leave estimation of the principal parameters of earthquakes to the United States Coast and Geodetic Survey. The advice was not taken, which is probably just as well. The USCGS no longer exists and though its Preliminary Determination of Epicentres service has survived, the US Geological Survey, which assumed this responsibility, could well be looking for a suitable opportunity to transfer this to an internationally supported body. World Data Centre A now satisfies most needs for seismograms and WDC B is being encouraged to be more effective.

The ISC, meanwhile, has effected a welcome facelift. There seems an end to the computing difficulties which have beset the ISC since its inception. The *Bulletin* of principal data now appears monthly in a computer typeset format. The lag time of four years has been reduced to two at the behest of the governing council, thereby allowing data from even the most inhospitably sited of 800 or so reporting stations to be included. There are now twenty-four members (trustees) represented on the governing council contributing annually some £60,000 in sums of £200 upwards towards the income of the centre in Edinburgh. The deed which established the ISC in its new structure was sealed by the hosts (the University of Edinburgh) and the Royal Society. The day-to-day work of the centre is carried out by an international staff of ten under a full-time director on leave of absence from the US Geological Survey. This healthy looking organisation has come a long way in the past four years.

The primary product of the ISC is a definitive listing of the principal data of some 12,000 earthquakes per year, having magnitudes of about m_b $4\frac{1}{2}$ and greater. This is more than double the number published by the PDE service after its six-week data cut-off. The centre's amplitude and supplementary phase material, as well as its number of contributing stations (which include a well distributed sample from the Soviet Union), is superior to all others. The cost of printing of the bulletin is now the principal cause of concern. The idea of publishing it as microfiches is understandably very unpopular even at a third of its cost, yet how often, and by how many seismologists, is the full suite of bulletin data used? Not often is the probable answer and, in any case, major users almost certainly prefer the bulletin tapes.

An idea which may not have been dusted off in recent years is the use of a microfiche bulletin together with the centre's *Regional Catalogue*. The catalogue contains hypocentres, origin times and magnitudes listed by region. To many users the catalogue is the most useful publication, providing a geographical sort for selecting small

groups of earthquakes (or explosions, which are separately catalogued). Detailed information may be subsequently extracted from the bulletin. At a quarter of the bulletin printing costs, the substitution of a monthly catalogue for the bulletin makes good sense. The bulletin could be purchased *en bloc* for national centres, or separately as required by individual users. This might satisfy the majority of seismologists and all earthquake engineers.

A most useful and, it is to be hoped, growing function of the centre is the provision of special services. Custom data sorting is one example, and the UNESCO contracts for the production of the *Manual of Seismological Observatory Practice* and the seismicity map of South-East Asia are others of the way skills of a highly professional group are being exploited. The centre also provides a home (and funds if the work is likely to be beneficial to the centre's principal task) for seismologists whose research at the time requires immediate access to the world's premier collection of earthquake data.

The central reason, however, why the ISC is internationally supported is for the organised data it publishes. The only advantage of the bulletin is that it produces under one cover what is already available by searching the records of a number of centres like the PDE service and the Soviet and BCIS bulletins. Although most seismologists feel that the saving in time, as well as the greater accuracy of the additional processed data, are sufficient reasons for the existence of the centre, others want searches made for hypocentres of earthquakes not reported elsewhere, and would like to see attempts made to include data descriptive of the seismograms. It is doubtful whether the latter can ever be a substitute for personal reading of the seismogram. The attempts which the centre is already making to invite fuller reporting of phase arrivals and amplitudes may well fill all reasonable needs. The location of earthquakes from a mass of unassociated P times is a more controversial subject. Judged by the annual number of published epicentres, it is unlikely that the bulletin misses many earthquakes of magnitude m_b $4\frac{1}{2}$ or more, particularly in the Northern Hemisphere, so the principal contribution would be events up to half a magnitude below this threshold where seismological interests are narrower.

The safest criterion whereby to define a generally useful threshold might be that of unbiased world seismicity, and especially to equalise the magnitude threshold of the southern (oceanic) half of the globe with the northern. Seismologists must decide and at present only those from a fifth of the countries most concerned have any substantive influence on the scientific policies of the ISC. Curiously, some of the most seismically affected countries are unrepresented; so are international oil and construction companies which consult the centre regularly. Between them they could put the welfare of the ISC beyond doubt.

P. D. M.

T Lymphocytes in Nude Mice ?

THE 'nude' mouse, homozygous for the gene *nu*, has been hailed as the ideal animal for studying the immunological consequences of athymia. Besides being topless and bottomless, it is generally considered to have, at the most, a minute and grossly abnormal thymus, and to lack thymus-processed ('T') lymphocytes. The article by Raff on page 350 of this issue of *Nature* therefore raises two important and related questions. First, are nude mice in fact totally devoid of T lymphocytes? Second, is the theta (*alias* Thy-1) alloantigen a genuine certificate of authenticity, conferred on no lymphocytes other than those which are passing through, or have graduated from, the thymic educational system?

What Raff has shown is that up to about 2% of lymph node and spleen cells from nude mice fluoresce when exposed to specific fluorescein-conjugated anti-theta antibodies. This proportion is very low in comparison with normal mice and implies that nudes possess perhaps 3% or less of the usual complement of theta-positive lymphocytes. The number of mice examined is small, but they come from three different sources, and comparable observations have been made in another laboratory (Lameline *et al.*, *J. exp. Med.*, **136**, 984; 1972).

The origin of the theta-bearing cells is still a matter for conjecture. The *nu/nu* thymus may perhaps show some transitory function in foetal life, allowing a few T lymphocytes to sneak through. Certainly, as shown recently by Pritchard and Micklem (*Clin. exp. Immunol.*, **14**, 597; 1973), nude mice possess progenitor cells which differentiate to thymocytes and functioning T lymphocytes in the environment of a grafted thymus. The possibility that T lymphocytes cross the placenta from the phenotypically normal (*nu/+*) mother is now open to test, since in germ-free or pathogen-free conditions *nu/nu* females can (contrary to previous belief) produce young. It is still not clear how much of the thymic educational process demands residential attendance within the thymus and how much can be achieved by postal tuition (thymic hormones or activation factors). Such factors would presumably cross the placenta more readily than cells.

This leads on to the second question. To rephrase it: is the theta antigen marker any use? As Raff makes clear, the antigen is not confined to thymocytes and T lymphocytes, but is present on epidermal cells, a component or components of the nervous system, and probably elsewhere. Apparently nude mice carry theta in these locations (Komuro and Boyse, *J. exp. Med.*, **138**, 479; 1973), and it is very unlikely that its appearance there depends on any thymic influence. By contrast, it is stated to be absent on lymphocytes of non-thymic lineage, and it is on this belief that its value as a marker depends. Boyse and his colleagues (most recently in *J. exp. Med.*, **138**, 1027; 1973) have found that theta may be induced to appear on the surface of previously negative cells (including *nu/nu* spleen cells) by a variety of *in vitro* tricks. These include brief exposure to thymus extracts of several origins, but also to other substances (bacterial endotoxin, polyadenylic-uridylic acid and cyclic AMP) whose relevance to the thymus is more debatable. So far, few functional tests on these induced cells have been reported. The modification of sensitivity to azathioprine in rosette-forming lymphocytes exposed to thymic factors

(Bach and Dardenne, *Immunology*, **25**, 343; 1973) may be a related phenomenon, but again the functional significance of the change is obscure. There is therefore no compelling reason at present to conclude that nude mice harbour any functional T lymphocytes.

Against the immunofluorescence data must be set experimental results from several laboratories, almost all of which suggest that thymus-dependent functions are absent. But it takes a refined experiment indeed to distinguish between profound depression and total ablation. For many purposes the distinction is probably not very important, but in situations where it may be critical one should, in the words of the old Scottish motto, 'gang warily'.

H. S. M.

Probing Lunar Carbon

CARBON compounds of biological significance have not been detected in lunar samples. Elucidation of the unusual chemistry of the element carbon in the non-biological lunar environment, however, continues to provide a fascinating challenge. Two of the analytical approaches which have been used to study the part per million concentrations of carbon present are pyrolysis and acid dissolution: pyrolysis gives a measure of total carbon without distinguishing its chemical state; acid dissolution measures the concentrations of gaseous hydrocarbons (CH_4 , for example) and carbide but gives no measure of total carbon. Both methods have shown that most of the carbon in lunar soil has derived from sources outside the Moon, principally implantation by the solar wind and, to a lesser extent, by impacting meteorites. Indeed, the amounts of carbon and its compounds give an indication of the degree of exposure of the constituent soil particles at the Moon's surface. The particles are carried through a cycling process of erosion and aggregation, mainly brought about by micro-meteorite bombardment. Large grains are eroded to smaller particles which are, in turn, aggregated into more complex grains, such as glass welded agglutinates. Carbon added from extralunar sources is originally accumulated at particle surfaces but, during aggregation, a proportion of the carbon species in the smaller particles is incorporated into the newly formed composite particles to give a volume-related carbon component.

In a recent issue of *Nature Physical Science* (**246**, 65; 1973), Desmarais *et al.* suggest that this volume-related component, measured by extrapolation of plots of total carbon against the reciprocal of particle radius, is a better indicator than total carbon of the time of exposure of a soil sample at the surface of the Moon. They believe that the rate of recycling by erosion and aggregation is faster than the rate of accumulation of carbon from outside the Moon and conclude that the carbon species found on the Moon are not directly relatable to source because of "blurring" caused by the rapid recycling process.

The outlook for the further unravelling of lunar carbon chemistry may not be as bleak as this hypothesis suggests, although the complete history of the constituent carbon atoms will probably never be known. Other lunar in-

investigators believe that more detailed sample analysis, including single particles, and simulation studies of ion implantation and meteorite impact will throw light on the respective roles of the solar wind and meteorites in bringing about the synthesis of the observed carbon compounds. Thus, in a recent contribution to *Nature Physical Science* (245, 3; 1973) Pilling et al. suggest that the chemically detected iron carbide present may have derived by dissolution of carbon in submicroscopic droplets of metallic iron; these Ångström sized droplets seem to have been formed from ferrous iron and solar wind implanted hydrogen during the flash heating induced by micrometeorite impacts.

The unusual synthetic reactions which occur during the evolution of lunar soil particles should therefore continue to provide a fascinating challenge in surface chemistry for some time yet. Just how to solve a number of crucial questions remains to be seen. For example—what is the chemical state of the major proportion of the carbon?

From a Correspondent

TROPONIN

A Molecular Latch?

from a Correspondent

MUSCLE contraction is known to involve the sliding of thick filaments past thin filaments. The control of the contraction by Ca^{2+} ions is thought, at least for vertebrate skeletal muscle, to involve changes in the structure of the thin filaments. Each thin filament is an assembly of three proteins—actin, tropomyosin and troponin—and the way in which these proteins interact and are controlled by Ca^{2+} is currently attracting considerable attention.

In the thin filament, long rod-like tropomyosin molecules are thought to be joined end-to-end to follow the two long-pitched strands of the actin structure, each tropomyosin molecule being associated with seven actin subunits. One molecule of the Ca^{2+} -sensitising protein troponin is attached to each tropomyosin molecule. Changes in the X-ray diffraction pattern of muscle on contraction have suggested that the binding of Ca^{2+} ions to troponin causes the string of tropomyosin molecules to move with respect to the actin structure from a position relatively far from the groove between the two actin strands to a position near the groove. One current hypothesis is that sites on the actin subunits are thereby uncovered and can then interact with the cross-bridges of the thick filament. Conversely, the

removal of Ca^{2+} ions from troponin causes the opposite movement of tropomyosin so that the actin sites are no longer available and relaxation is possible.

In order to understand how this movement of tropomyosin can occur, the interactions which troponin makes with actin and tropomyosin need to be defined. The problem is further complicated by the fact that the troponin molecule contains three kinds of subunit—troponin-T (molecular weight 44,000 in chicken), troponin-I (mol. wt 23,000) and troponin-C (the calcium-binding subunit with a molecular weight of 19,000). All three subunits are required for relaxation in actin-myosin-tropomyosin systems. Which of these troponin components binds to actin and which to tropomyosin?

For some time, it has been appreciated that the troponin-T subunit can bind to tropomyosin (which is why the subunit is so named). Hitchcock, Huxley and Szent-Györgyi (*J. molec. Biol.*, 80, 825; 1973) now produce new evidence that even when troponin-T is excluded, a complex of the other two subunits, troponin-I plus troponin-C, can bind quite strongly to actin-tropomyosin provided that Ca^{2+} ions are absent; in the presence of Ca^{2+} ions the binding is poor. Tropomyosin itself, however, cannot bind the complex of troponin-I and troponin-C; this is deduced from the fact that the appearance of tropomyosin paracrystals or the viscosity of tropomyosin in solution is not altered in the presence of these components. Since troponin-C by itself has no affinity for either tropomyosin or actin, the troponin-I subunit must be responsible for the binding to actin. This result fits in well with observations by other groups (notably that of Perry) that troponin-I can inhibit actomyosin ATPase in the absence of tropomyosin.

Hitchcock et al. propose as a working hypothesis that each troponin molecule is bound to the thin filament at two sites: one involves a strong Ca^{2+} -independent interaction between the troponin-T subunit and tropomyosin; the other involves a Ca^{2+} -dependent interaction between the troponin-I subunit and actin. Both interactions are required to obtain relaxation. Calcium, by binding to troponin-C, in some way reduces the binding of troponin-I to actin but has no effect on the binding of troponin-T to tropomyosin.

It has always been difficult to imagine how if troponin bound only to tropomyosin it could cause relative movements of actin and tropomyosin on binding Ca^{2+} ions. Now that there is evidence that troponin can also bind to actin in the absence of Ca^{2+} ions, it is possible to speculate how this movement might be brought about. For example, in the absence of Ca^{2+} , the

tropomyosin might serve to secure the tropomyosin molecules to the actin structure in the position producing relaxation, whereas in the presence of Ca^{2+} the troponin would no longer bind to actin and the tropomyosin molecules would be free to adopt the position producing contraction. At all events, with five polypeptide components and at least five types of protein-protein contact the thin filament clearly provides an abundance of interactions for the allosterically-minded.

JUVENILE HORMONE

Delayed Effects

from our Insect Physiology Correspondent

ONE of the most promising effects of the juvenile hormone and its synthetic mimics, considered as potential agents for pest control, has been the disruptive action they can exert on subsequent development when applied to the pregnant female or to the developing eggs. In some insects not only can embryonic development be so upset that the eggs fail to hatch, but apparently normal larvae may show signs of excessive juvenile hormone production in the later stages of development: they may give rise to non-viable 'adultoids', or intermediates between larva and adult, at the time when they would have been expected to undergo normal metamorphosis.

This delayed effect was at first attributed to the persistence of traces of the active substance as a contaminant of the egg shells from which the treated insects had emerged. But Riddiford and Truman (*Nature*, 237, 458; 1972) proved convincingly that in the linden bug *Pyrrhocoris apterus* there is a lasting disturbance in the secretory activity of the corpus allatum in the treated insects. It seems that in such insects the corpus allatum continues to secrete juvenile hormone in the fifth instar and thus to maintain larval characters when the insect should have become adult. Excision of the corpus allatum from such larvae led to normal metamorphosis; transplantation of their corpus allatum into normal fifth instar larvae caused these to transfer into adultoids.

Effects of this sort do not occur in all insects. In the cabbage butterfly *Pieris* there is no such delayed effect on metamorphosis. It has now been shown by Hunt and Shappirio (*J. Insect Physiol.*, 19, 2129; 1973) that in the lesser milkweed bug *Lygaeus kalmii* the excessive hormone secretion, induced by the application of juvenile hormone analogues to the developing embryo at about the time of blastokinesis, will not only prevent proper metamorphosis of the fifth instar but will lead to the distinctive colour pattern of the early larval instars being retained in the later

instars—a result which supports the view that the morphological changes during development, even before metamorphosis, are controlled by the level of juvenile hormone secretion.

The long delayed effects of hormone treatments during the perinatal period are a matter of concern for clinicians (*Science, N.Y.*, **181**, 189; 1973) because it has been found in vertebrates that steroids and other hormones administered at the time of birth can induce malignant tumours, as well as changes in behaviour, in adult life.

CANCER RESEARCH

Tokyo Meeting

from a Correspondent

THE 4th International Symposium of the Princess Takamatsu Cancer Research Fund, which opened in Tokyo on November 6, discussed differentiation and control of malignancy.

G. Tompkins (University of California, San Francisco) began the meeting by reporting that nuclear sites can distinguish between two cytoplasmic steroid receptors, which bind to DNA or chromatin, but not to RNA. S. Ohno (City of Hope Medical Center, California) demonstrated the simplicity of mammalian regulatory systems with an elegant example from the (*Tfm*) androgen-receptor protein locus on the X chromosome of the mouse. The mammalian embryo, he suggests, has an inherent tendency to develop as a female; male development must be induced by an androgen. Therefore, he says that the X-linked *Tfm* locus is the one and only regulatory locus which determines sexual phenotypes of mammals.

J. Paul (Beatson Institute, Glasgow) described experiments confirming the thesis of a positive regulatory role for non-histone chromosomal proteins. Non-histone proteins from erythroid cells allow brain chromatin to transcribe globin genes. The mechanism of control of transcription was discussed in the light of Paul's recently published model for the structure of chromatin. This proposes that the transcriptional unit in eukaryotes is similar to that in prokaryotes and that non-histone proteins bind to or near promoters to permit the attachment of RNA polymerase which cannot otherwise enter the DNA-histone supercoil.

The new nuclear polymer, poly (ADP-ribose), was introduced by S. Shall (University of Sussex). He described the general properties of this polymer which is made by a chromosomal enzyme; NAD⁺ is the substrate, DNA and histone stimulate the activity of the synthetic enzyme, cyclic AMP inhibits the enzyme which hydrolyses the polymer and the polymer seems to be covalently attached to chromosomal

proteins. The specific activity of the synthetic enzyme fluctuates markedly in lymphoid cells during the transition from the non-growing to the growing state. Also, it varies during both the cell generation and the growth cycle. There is some evidence that it is involved in the regulation of DNA replication. The physiological function of this chromosomal enzyme system is unknown at present. Shall suggested that these enzymes of nuclear NAD metabolism directly integrate NAD and cellular energy metabolism with DNA biosynthesis both in progression through a single cell generation and in modulation of growth rate. In particular, he suggested that poly (ADP-ribose) may function by regulating the initiation of succeeding sets of replicons.

An explanation for the Pasteur and Crabtree effects was proposed by S. Weinhouse (Fels Research Institute, Philadelphia) who suggested that competition between pyruvate kinase and mitochondrial respiration for available ADP could account for these two phenomena.

A very simple and ingenious technique for the detection of abnormal differentiation of the gastric mucosa has been developed by T. Sugimura and colleagues (National Cancer Centre, Tokyo). It uses diagnostic Test-tapes to detect disaccharides in gastric mucosa. Investigations with cyclic AMP and the differentiation of neuroblastoma cells in culture suggested to K. N. Prasad (University of Denver) that a chemotherapeutic regime worth investigation would be a combination of sodium butyrate, dopamine and a cyclic AMP phosphodiesterase inhibitor.

Induction of functions by 5-bromo-deoxyuridine (BUdR) is being increasingly useful. H. Koyama and T. Ono (Cancer Institute, Tokyo) described the induction of alkaline phosphatase for which DNA synthesis is required. Melanoma cells become contact-inhibited and unpigmented in BUdR (S. Silaghi, Cornell University, NY). DNA synthesis and cell division seem to be obligatory. Tumorigenicity is also partially or completely suppressed.

Studies with cell hybrids are very informative, but the general tenor of the reports at this meeting called for caution and prudence in interpreting data and in drawing general conclusions about the nature of malignancy and its suppression.

M. Hozumi and colleagues (National Cancer Centre, Tokyo) are studying a protein factor which induces differentiation with a loss of tumorigenicity in cultured leukaemia cells.

The meeting ended with a thought-provoking discussion of the general nature of cellular differentiation and its relationship to malignancy which was sparked off by H. Holtzer (University of

Pennsylvania). This was a welcome addition to the attention to particulars with which most scientific meetings rightly concern themselves.

This meeting was stimulating and very informative. Its success is due tribute to the excellence of Japanese science and to the wisdom of Imperial patronage of science and especially of cancer biology, biochemistry and molecular biology.

PESTICIDES

Pests on File

from a Correspondent

THE use of computers in pesticide research was considered at a joint meeting of the Pesticides Group and Physicochemical and Biophysical Panel, Society of Chemical Industry, on October 15.

Aspects of modelling the behaviour of pesticides and pests were first considered. The panel chairman, I. J. Graham-Bryce (Rothamsted Experimental Station), stressed in his introduction that models are greatly simplified compared with the real situation and assumptions must be selected carefully and, ideally, confirmed. The sensitivity of the model to various factors can then be explored and thus critical ones can be identified. R. C. Bridges (Plant Protection Ltd, Jealott's Hill Research Station) considered penetration of a pesticide applied to the surface of a wheat leaf and its subsequent movement through the xylem. Several pathways into the leaf are feasible and the equations for the multi-compartmental model can be solved on a digital computer. The permeability of the pesticide in the leaf is critical: too rapid penetration can lead to accumulation and possible phytotoxicity; too efficient transport through the xylem may concentrate the compound in the distal parts of the lamina and provide insufficient protection to the remainder.

J. M. Osgerby (Shell Research Ltd, Woodstock Laboratory) considered applications of the analogue computer, which can simulate physical systems, to the addition of the molluscicide trifenmorph to water in an irrigation canal complex in Egypt (*Pestic. Sci.*, **1**, 5; 1970). Trifenmorph was added at one point upstream of the area to be treated. The main feeder canal carried water continuously to storage dams from which small canals ran to fields that were irrigated for 10 h daily from Monday to Friday, inclusive. By varying factors such as dosage, the time and duration of the trifenmorph, the optimum conditions were deduced within the limits of the model. Osgerby also considered examples of the diffusion and leaching of pesticides in soils treated with various formulations (*Pestic. Sci.*, **3**, 753; 1972).

Following work at the Glasshouse Crops Research Institute, the predatory mite, *Phytoseiulus persimilis*, is widely used in the United Kingdom to control the glasshouse red spider mite, *Tetranychus urticae*. H. G. Fransz (Netherlands Institute for Marine Research, Texel) discussed three ways of simulating the predation of *T. urticae* eggs by *Typhlodromus occidentalis*, which is used in Holland, considering factors such as prey density and predator gut content. His stochastic approach, in which the system changes state according to a series of random numbers, was run 100 times for each prey density to determine the mean values and 95% confidence limits. In his deterministic simulation random events were replaced by continuous rates of change and a hypothetical mean run once for each prey density and so forth. In his compound method gut content was divided into five states and a deterministic model applied to each (and to other factors split into states). For given conditions he found differences in the number of eggs attacked between stochastic and deterministic models with the compound method nearer the second model.

The use of computers in information storage and retrieval was then considered with R. A. E. Galley in the chair. J. A. Silk (Plant Protection Ltd, Jealott's Hill) discussed the value of computers in technical information services, in providing information summaries on which management decisions can be based and explained how relevant references to published literature are provided from commercially available tapes. Clearly for an organisation the size of Imperial Chemical Industries computerised sorting and retrieval of information are essential, a point reinforced by G. Palmer (ICI, Pharmaceutical Division), who stated that more than 135,000 compounds are stored in the company's file with details of their biological activity, availability and Hansch parameters. He explained how their structures are stored in the Wiswesser Line Notation (WLN), letters and numbers standing for specific chemical groups. When the memory is searched for compounds of the required type on an atom for atom basis the rate is only 350 compounds per minute, so a preliminary sorting is needed to reject most unwanted compounds, for example a one fragment search runs at 50,000 and a WLN search at 4,000 compounds per minute.

D. T. Sagers (Fisons Ltd, Agrochemical Division) explained that testing compounds for biological activity enabled those with desirable properties and the relationship between activity and chemical structure to be identified and he illustrated this by structures (again in WLN) with specified pesticidal

activity and recalled by computer. J. C. Wickham (Wellcome Foundation, Berkhamsted) showed how computers can be used to undertake probit analysis, to determine the relative potencies of a series of related compounds and to predict the properties of aerosol mixtures, containing, for example, an insecticide, a synergist and a knock-down agent. Costs of the components can be included in the program and the commercially optimum recipe can be decided.

H. S. Stammers (Shell Research Ltd), introducing the discussion, asked whether too much of computer time is spent on statistical analyses, the results of which are of questionable value, and warned that the study of model systems can become expensive. Several of the audience agreed with earlier speakers that the next stage is a greater use of on-line computers which can be interrogated directly.

IRON-SULPHUR PROTEINS

Low Redox Potentials

from our Photosynthesis Correspondent
CAN membrane bound iron-sulphur proteins with midpoint redox potentials lower than -530 mV be involved in the primary photochemical events of chloroplast photosynthesis? This is a question posed from recent work of Ke, Hansen and Beinert (*Proc. natn. Acad. Sci., U.S.A.*, **70**, 2941; 1973). It is about 3 years since Malkin and Bearden

(*ibid.*, **68**, 16; 1971) first suggested from electron paramagnetic resonance (EPR) studies that a membrane bound ferredoxin may serve as a primary low potential electron acceptor for photosystem 1 (S1). Since then work from several laboratories has supported the idea that an iron-sulphur centre accepts electron from the light activated S1 reaction centre (P700). Several lines of investigations have indicated that the redox level of the reduced iron-sulphur centre was lower than -250 mV and could possibly be -470 mV.

By carrying out redox titrations with digitonin-fractionated S1 subchloroplast particles monitored by low temperature (13 K) EPR spectroscopy, Ke and his colleagues have uncovered some new and striking properties of membrane bound iron-sulphur proteins. They have resolved three different iron-sulphur centres: one with resonance lines at $g=2.05$ and 1.94 ; one with lines at $g=2.05$, 1.92 and 1.89 ; and one detected by a single line at $g=1.86$. Redox titrations between -450 and -610 mV carried out at pH 10 indicated that the midpoint potentials of these iron-sulphur species fall into two distinct separate regions, -530 ± 5 mV and 580 mV. The redox reactions of each of the membrane bound iron-sulphur centres were found to be reversible and apparently involved a two electron change. Although all three centres could be reduced by light at room temperature, at 77 K illumination did not bring about photoreduction beyond that

Magnetic Fields in Neutron Stars and White Dwarfs

In a supernova explosion the interior of a star may be imploded to form a neutron star or white dwarf and, as the electrical conductivity of stellar material is high, magnetic flux should be conserved during the implosion. As a result an initial field of only 100 gauss can be converted to 10^{12} gauss in the resulting neutron star. If pulsar neutron stars are typical, neutron stars do have magnetic fields of strength about 10^{12} gauss. In addition a small minority of white dwarfs have strong fields of order 10^7 gauss. Although flux conservation during implosion may account for the neutron star fields, it is perhaps surprising that most pulsar neutron stars seem to have magnetic fields of about the same strength, for stellar fields before implosion may have very different strengths.

Present theories of supernovae produced by explosive carbon burning suggest that a remnant white dwarf or neutron star can only be left if the onset of the explosion is delayed by the occurrence of convection in the interior of the star. Without cooling by convection, the explosion is catastrophic

and no remnant remains. In *Nature Physical Science* next Monday (December 10), Ruderman and Sutherland suggest that this convection produces the magnetic field which is amplified in the final stages of implosion. They suggest that convection will amplify any initial field until its energy is in equipartition with the energy of the convective motions. Using estimates of convective velocities based on the mixing length theory of convection, they predict a field of 10^{12} gauss in neutron stars and one of order 10^7 gauss in what they take to be a small minority of white dwarfs formed in supernova explosions.

This is a very promising suggestion although some of its details are controversial; for example, the mixing length theory may not give a very good estimate of convective velocities. It has never been proved convincingly that an equipartition field must be produced by the interaction of magnetic fields and convection and the field produced by such interaction might be a small scale field rather than a large scale field of strength 10^{12} gauss. These points will no doubt be discussed in future.

accomplished electrochemically, at about -560 mV.

These observations are particularly striking since it would place the iron-sulphur species detected by Ke *et al.* at the strongest reducing level known for a biological electron carrier. So far it is difficult to assess the involvement of all these species in primary S1 activity especially because of the temperature sensitivity of the iron-sulphur centres with midpoint potentials beyond -560 mV. Bearing this in mind the results indicate that the chemical potential, generated between the primary donor (P700) and primary acceptor of S1 due to the absorption of photon energy, could be at least 0.96 or possibly 1.01 eV.

SPACE RESEARCH

Observing Satellites

from a Correspondent

A DISCUSSION meeting on satellite observations and their use in orbital analysis was held at the Royal Society on November 17. The meeting, organised by the Optical Tracking Subcommittee of the British National Committee on Space Research, was designed to bring together the volunteer visual observers of artificial satellites, whose observations (some 25,000 per year) are so valuable in determining satellite orbits for subsequent analysis in geophysical research.

The meeting began, appropriately enough, with a talk on the visual appearance of satellites by D. M. Brierley (Royal Radar Establishment, Malvern), who emphasised that non-spherical satellites can suffer great variations in brightness when the solar phase angle is unfavourable. J. A. Pilkington (Scarborough Planetarium) discussed some of the difficulties in finding reliable information for listings of newly-launched satellites, and some old ones too. Recent geophysical advances resulting from analysis of orbits were described by D. G. King-Hele (Royal Aircraft Establishment, Farnborough): he mentioned the new determination of the Earth's pear-shaped polar section (*Nature*, **246**, 86; 1973), and discussed the evaluation of fifteenth-order harmonics in the geopotential from analysis of orbits at fifteenth-order resonance; good values have now been obtained for harmonics of order 15 and degree 15, 17, 19 . . . 27. Next G. E. Perry (Kettering Grammar School), accompanied by a tape recorder, regaled the audience with a medley of interesting radio signals from recent satellites caught during his untiring pursuit of transmissions from satellites.

The observers attending the meeting had previously answered a comprehen-

sive questionnaire running to seventy-eight questions, prepared by M. D. Waterman (Elliott Automation, Frimley), who reported some interesting features of the statistics. As might be expected with observations relative to the stars, the results of the survey showed that observational accuracy improves with experience: observers are at their best after more than 10 years' practice. An astonishing contrast to this finding was provided by sightings of the flaming decay of satellites: the regular observers at the meeting had made more than a quarter of a million observations and had scanned the night sky for thousands of hours, yet only one of them had seen a decay; but the minority of the audience who were not regular observers included four defiers of the laws of chance, who had seen a decay. There were further contributions from A. J. G. Moorat (Appleton Laboratory, Slough), on satellite predictions; from G. E. Taylor (Royal Greenwich Observatory), on time standards; from R. D. Eberst (Royal Observatory, Edinburgh), on recent work at Edinburgh; and from J. Hewitt (Ordnance Survey), who gave a very clear description of the techniques for laser tracking of satellites.

EARTHQUAKE PREDICTION

No Dilatancy

from our Geomagnetism Correspondent

BECAUSE of bad communications, early Russian work on the changes of seismic velocity associated with earthquakes took a long time to become known in the west, and even more surprisingly in Japan. Thus although as long ago as 1962 Kondratenko and Nersesov (*Trudy Inst. Fiz. Zemli*, **25**, 130; 1962) had reported that the velocity of P waves in the crust increases after moderately large earthquakes in the Tadzhikistan region of southern central Asia, this secret apparently remained locked inside the Soviet Union until Savarensky (*Tectonophysics*, **6**, 17; 1968) reviewed the results in English. Moreover, even then the Russian studies were treated with some scepticism. Rikitake (*Earth-Sci. Rev.*, **4**, 245; 1968), for example, argued that the 30% change in elasticity implied by a reported 15% change in P wave velocity threw doubt on the validity of the original data.

Nevertheless, the appearance of Savarensky's report, other reports of Russian work, and particularly an article by Semenov (*Izv. Acad. Sci. USSR Phys. Solid Earth*, **4**, 245; 1969) giving details of premonitory seismic velocity changes, inspired United States seismologists to search for similar effects. Semenov had shown that before earthquakes in the magnitude range 3–5, the

velocity ratio v_p/v_s decreased by about 6% and then increased back to its normal value just before the main shock. A few years later, Aggarwal *et al.* (*Nature*, **241**, 101; 1973) reported similar results involving decreases in v_p/v_s of up to 13% for New York earthquakes in the magnitude range 1–3. But the really important result was obtained recently by Whitcomb *et al.* (*Science*, N.Y., **180**, 632; 1973), who showed that 4 years before the 1971 San Fernando earthquake of magnitude 6.4 the v_p/v_s ratio decreased suddenly by 10% from its average value of 1.75 and then gradually rose back to about the average value at the time of the main event. Equally importantly, Whitcomb and his colleagues were able to propose a plausible physical mechanism for these changes in terms of rock dilatancy.

Whitcomb *et al.* also found that the greatest contribution to the change in v_p/v_s occurred in v_p and thus argued that, because man-made explosions produce mainly P waves, the path to earthquake prediction could be opened up by the use of artificial sources "such as large mine blasts" rather than being limited to the vagaries of earthquakes. This challenge was taken up by McEvilly and Johnson (*Science*, N.Y., **182**, 581; 1973), who have examined the travel times of P waves from Californian quarry explosions of well known origin times. The results are disappointing to say the least. To put it bluntly, McEvilly and Johnson find no velocity changes associated with earthquakes at all.

The quarry at which the sixty-eight examined explosions occurred between July 12, 1961, and June 14, 1963, lies near Salinas, California. The P waves from the blasts were recorded at the University of California's seismic stations, some of which have been in operation continuously since the early 1960s and the nearest of which lies about 13 km from the quarry (equivalent to a P_g wave travel time of 2.5 s). The uncertainties in reading the P wave arrival times were estimated to be less than 0.1 s, although there were, of course, other possible sources of variation in travel times. For example, some of the blasts were not timed at the quarry, and their origin times had to be inferred to within 0.1 s on the basis of arrival times at the nearest station. But McEvilly and Johnson estimate that the maximum error possible, including that arising from a 10% dilatancy-induced velocity change over the distance to the nearest station, would give arrival times at a particular station within a range of ± 0.4 s about a mean value. Thus for mean travel times of 8.6–28.0 s to the seven stations used (excluding the nearest station), the extreme variations would be $\pm 5\%$ at the nearer stations and $\pm 1.5\%$ at the more distant stations.

In fact, the travel times and velocities to all seven stations were all within $\pm 2.0\%$ of their respective mean values, and most were within $\pm 1.0\%$. Moreover, the measurements with the largest deviations from the means are most easily accounted for by known sources of error such as source locations and origin times. And, finally, a comparison with the times of occurrence of earthquakes in the magnitude range 4.5–5.5 gave no indication whatsoever of any premonitory velocity changes prior to the earthquakes.

McEvilly and Johnson suggest four possible explanations of these results. The first is that although the dilatancy effect exists, it was not observed in this case because of poor sampling in time. But this argument is held to be weak because, notwithstanding the sampling problem, at least one change of greater than 2% would have been expected, especially bearing in mind the occurrence in the area of a large number of earthquakes with magnitudes less than 4.5. The second explanation also assumes that the dilatancy effect exists but that it is obscured by poor sampling in space. This requires that the effect of a velocity decrease of at least 10% in the earthquake source region should be such as to give an overall velocity change of less than 2% out to the recording stations; and this in turn requires that the region affected by dilatancy in the vicinity of magnitude 4.5–5.5 earthquakes should be less than 10–20 km in diameter. Such restrictions, however, do not seem to be impossible.

The third possibility is that the region investigated is uniformly dilated as a result of either the usual seismicity in central California or of an impending major earthquake. The main argument against such a situation is that the v_p/v_s ratio seems to be "normal" at about 1.75. The final explanation proposed is simply that dilatancy is not an important effect at all in the vicinity of a major strike-slip fault, possibly because either the stress levels are insufficient to cause dilatancy or, if sufficient, are more or less stable without large fluctuations.

So where does that leave the general question of premonitory seismic velocity variations? The answer is probably that there has been little real change. These new results must certainly temper the euphoria induced by the Whitcomb report; but at the same time they emphasise that the data are insufficient to support either of the extremes of pessimism or optimism. Assuming that one snowflake makes a winter is no more justified than seeing the summer in a single swallow. The fact is that although the McEvilly–Johnson results are consistent with the absence of the dilatancy effect, there seems to be at least one explanation which does not

preclude dilatancy. On balance the evidence is still somewhat in favour of the existence of premonitory seismic velocity changes.

MATERIALS

Fatigue in Composites

from a Correspondent

THE subject matter of the twelve contributions presented at a symposium on fatigue in composites organised by the Institute of Physics at Imperial College, London, on November 15, ranged widely. The fields covered included the behaviour of metal matrix composites reinforced with metal fibres and with whiskers; fatigue resistance of a variety of carbon fibre and glass fibre reinforced plastics, laminates and joints under different forms of loading; the setting up of safe-life design criteria for the use of composites in realistic conditions; and non-destructive testing

techniques for studying damage sustained during testing and in service. Both in prepared papers and in discussion it was clear that three principal topics claimed the attention of the participants, both investigators and users: not surprisingly, these three are fundamentally interrelated.

The first topic concerns the question of variability of fibre and composite properties, and defects in manufactured materials. J. Sturgeon (Royal Aircraft Establishment, Farnborough) discussed the importance of using failure probability analysis in order to obtain usable stress/life curves for fatigue of carbon fibre reinforced plastics (CFRP). From discussion following his remarks and also those of A. K. Green (Imperial College, London), it became clear that it is not generally possible to extrapolate results from one batch of material to another, or from one laboratory to another. This fact considerably impedes the procedures of developing and using

Sinistral Movement Along the Great Glen Fault

WHAT movement has taken place along the Great Glen fault? This is a question which has received a variety of answers, most of which conflict with each other. When Kennedy (*Quart. J. Geol. Soc.*, **102**, 41; 1946) first recognised the fault as transcurrent, for example, he assumed that the Caledonian Strontian and Foyers granitic complexes had once been part of the same intrusion and concluded from this that a sinistral displacement of 100 km had occurred. A year later he found further support for the same conclusion from the matching of the metamorphic zonation in the Moinian assemblage rocks. But during the past few years, not only have differences in the Strontian and Foyers intrusions been found, thus throwing doubt on the identity between the two, but the grades of metamorphism in the Moinian rocks surrounding the two intrusions have also been found to differ.

Going to the other extreme, Holgate (*Scott. J. Geol.*, **5**, 97; 1969) has adduced evidence for a Mesozoic dextral displacement of 30 km along the Great Glen fault, and Garson and Plant (*Nature*, **240**, 31; 1972) have even more recently claimed that the Caledonian movement was dextral. Clearly someone is wrong somewhere; but one of the difficulties in identifying who it is, or who they are, is that data are so often so incomplete that they may be taken to support directly opposing views simultaneously. Nor has geophysical evidence been of much direct help apparently. On the other hand, geophysical data have had something to say about which other faults are, or are not, continuations of the Great Glen fault. This is particularly relevant to the

Leannan fault in Ireland whose well-established sinistral displacement of 40 km circumstantially supports a sinistral movement along its Scottish counterpart.

This is part of the background against which J. A. Winchester, in *Nature Physical Science* next Monday (December 10), adduces evidence to support a sinistral displacement of 160 km along the Great Glen fault. What Winchester has done is to investigate the variations in metamorphic grade in the Moinian assemblage on each side of the fault and thus produce a metamorphic zonal map—a feat made possible by the widespread distribution of calc-silicate gneisses in the assemblage. In other words, he has been able to map the regional variations of metamorphic grade on each side of the fault in terms of the mineralogical variations in the calc-silicate gneiss bands and the distribution of gneissose injections.

The characteristics of the zonal map are too complicated to be summarised here; but what can be said in general terms is that the variations in metamorphic grade, and in particular the maxima and minima, can be adequately matched across the Great Glen fault only if a sinistral movement of 160 km is assumed to have taken place. The features of the match are numerous; but the most dominant example is that the high grade rocks of the Western Highlands complex and the Monadhliaths injection complex, both of which are truncated diagonally by the fault and both of which have about the same width at the fault line, are brought into juxtaposition when the pre-displacement pattern is reconstructed.

design criteria for fatigue in composites. The question hinges, as M. J. Owen (University of Nottingham) pointed out, on one's inability to define exactly the nature of defects in reinforced plastics and their likely effect on static and cyclic properties. N. J. Parratt (Explosives Research and Development Establishment, Waltham Abbey) observed that although in whisker reinforced aluminium alloys the minute whiskers do not *per se* act as flaws or crack starters, unfortunately it happens that the effects of small defects in such materials are larger than could be expected from fracture mechanics investigations on larger, more easily studied defects.

The second topic concerns the appropriateness or otherwise of the experimental work that is currently being carried out. Owen took up the point made by a member of the audience, that uniaxial fatigue tests on simple aligned or biaxially-oriented composites were hardly likely to provide information that would be useful for realistic design problems. He showed how a fracture mechanics approach could be used to develop safe-life design criteria for notched plates of glass reinforced plastics. A. W. Cardrick (RAE, Farnborough) gave added weight to these arguments when, during his paper on the properties of CFRP/aluminium composite joints, he listed as being most urgently required information on the effects of secondary stresses and stress concentrations, accidental damage, joint design and cumulative damage characteristics. He also emphasised that constant amplitude and frequency loading conditions were not as useful as narrow band random loading.

The third important question—fatigue damage—was also discussed by W. S. Carswell (National Engineering Laboratory, East Kilbride), who described an approach to defining residual strength of fatigued composites in terms of a damage function which, however, is extremely difficult to define, is even more difficult to measure and is likely to be different in different types of composite. Of particular importance in this respect were the contributions of W. N. Reynolds (Atomic Energy Research Establishment, Harwell), who discussed non-destructive testing methods as a means of assessing fatigue damage, and those of C. Kim (Chelsea College, London) and of M. Fuwa (University of Sussex), who showed how acoustic emission detection techniques can be used to give information about the damage sustained during loading and cycling of test pieces or components. It could well be that these non-destructive testing methods will have to be used in conjunction with design criteria in order to overcome or allow for the deleterious effects of variability and manufacturing defects.

NUCLEAR THEORY

Heavy Ion Transfer

from our Nuclear Theory Correspondent

MANY studies of nucleon transfer reactions between heavy ions have shown typically bell-shaped angular distributions centred at angles around 30° . These distributions can easily be understood using a classical model of the interaction. The lighter of the two ions approaches the heavier along a Coulomb hyperbolic orbit and the reaction in which a nucleon is transferred from one ion to the other is most likely when this orbit just grazes the surface of the heavier ion. For larger impact parameters the lighter ion passes beyond the nuclear field of the heavier ion and no nuclear reaction can take place. For smaller impact parameters the ions interact strongly and many complicated reactions take place, so that the probability of the simple transfer reaction is again reduced. Thus there is a maximum in the cross section at the angle corresponding to the deflection characteristic of a grazing collision, and it falls for smaller and for larger angles giving the observed bell-shaped curve.

Quantum mechanical analyses of such reactions can be made using the distorted wave theory and taking into account finite range and recoil effects. These are generally in agreement with the data but in some cases they have shown rapid oscillations superposed on the overall bell-shaped curve. It thus becomes interesting to see if these rapid oscillations can be detected experimentally and also if they can be understood theoretically. This has recently been done by a group at Brookhaven National Laboratory (Chasman, Kahana and Schneider, *Phys. rev. Lett.*, **31**, 1074; 1973).

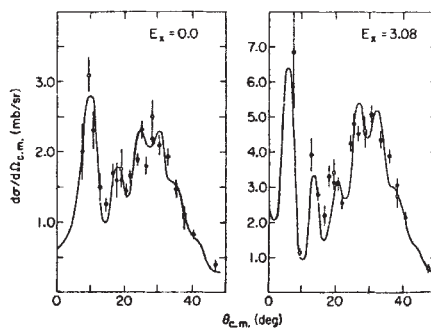
They measured the differential cross section for the proton-transfer reaction $^{48}\text{Ca}(^{14}\text{N}, ^{13}\text{C})^{49}\text{Sc}$ to the ground and 3.08 MeV states of ^{49}Sc at an incident energy of the nitrogen ions of 50 MeV. Preliminary data (open circles in the figure) showed the expected bell-shaped curve at 30° but with an additional peak at about 10° . The first distorted wave calculations gave a smooth bell-shaped curve without the forward peak but it was found that if the absorbing part of the optical potential describing the motion of the incident particle is slightly reduced, the forward peak appears and in addition oscillations of higher angular frequency (about 6°) appear on the bell-shaped curve.

Further measurements were therefore made to see if these oscillations are real, and the solid points on the figure show that this is indeed the case. The distorted wave calculations reproduce these complicated oscillations remark-

ably accurately, confirming the reliability of the calculations.

The physical origin of these small-angle oscillations is still not understood. Diffraction theory suggests that they must be associated with a distance of the order of the diameter of the target nucleus, so the obvious explanation is that they are due to interference between the projectile paths on opposite sides of the target nucleus. This is confirmed by calculations starting with the quantum mechanical expression for the cross section and using approximate analytical forms for the partial wave amplitudes and phases. It is then possible to obtain an expression very similar to those of classical diffraction models consisting of two terms, corresponding to interactions on opposite sides of the nucleus. The form of the cross section is then determined by the relative magnitudes of these terms, which varies with the incident energy and the interaction potential.

At low energies the classical scattering angle corresponding to grazing collisions is large, the two terms do not interfere and the classical bell-shaped cross section results. At higher energies the scattering angle is less, and the small-angle oscillations appear superimposed on the classical cross section. The onset of these oscillations depends on the absorbing potential. The lower this potential the lower the energy for which the incident particle feels the real nuclear potential and so the lower the energy at which the oscillations appear.



Differential cross sections for the proton transfer reaction $^{48}\text{Ca}(^{14}\text{N}, ^{13}\text{C})^{49}\text{Sc}$ to the ground and 3.08 MeV states of ^{49}Sc at an incident nitrogen energy of 50 MeV, compared with distorted wave calculations which include recoil.

The distorted wave calculations also allow the absolute magnitude of the transfer cross section to be related to the spectroscopic factor for the single-particle transition. The values extracted from the experimental results are found to be in excellent agreement with the nuclear structure calculations of Cohen and Kurath.

This work shows nuclear transfer between heavy ions is very well understood theoretically, so that it is now a powerful method of obtaining information on nuclear structure.

The Individual and the Information Problem

K. L. BLAXTER

The Rowett Research Institute, Bucksburn, Aberdeen

MILDRED L. BLAXTER

Centre for Social Studies, University of Aberdeen

This article deals with the needs and habits of scientific authors and readers.

THERE is little doubt that the cost of providing scientific information is increasing. In the six years ending in 1972 the mean cost of medical journals¹ has increased by 64% and that of general scientific and technical journals by 48% both increases being greater than that of general retail prices² which increased by 38%. The total purchases by scientific libraries have increased even more, for more journals are now being bought. Analysis of the costs of eight science libraries which in 1972 spent £38,000 on journal and book purchases shows they are well over double what they were six years ago.

Those who request librarians to purchase information are members of the community who produce the information. The whole system is thus a closed one, a matter which those concerned with commercial aspects of journal production have not been slow to realise. The system has been much studied by information scientists, indeed a new discipline 'bibliometrics' has emerged as a result³, and most of this work has confirmed and extended de Solla Price's earlier analysis of science as a totality^{4,5} to show that growth of information is exponential and parallels the increase in size of the scientific community. An alternative approach is to study the habits and assess the views of the individuals who contribute to the totality; this at least begins to define the operational needs of scientists. In addition such an approach enables an exploration to be made of the relation between the individual and the whole scientific community, for publication of the individual's work is the main link in this relationship. Whether growth of the mass of publications, the fragmentation of disciplines, the emergence of new sub-disciplines, and the fact that most scientists are paid employees, affect the concept of a universality in science are equally relevant questions.

Aberdeen Studies

For these reasons we have made a series of studies within three biological research institutes in Aberdeen, each of which publishes a report which describes its work. These, the Rowett Research Institute, the Macaulay Institute for Soil Research and the Marine Laboratory, deal with a series of disciplines relating to marine, soil, plant and animal studies. The workers have but a small educational commitment; their views and habits may well differ from those of

university lecturing staff, and for this reason they are called 'working scientists'. No logical inferences should be drawn from this terminology. The three institutes together publish 150 primary scientific papers (excluding reviews and popular articles) per annum, and the mean number of authors per paper ranges from 1.7 for the Marine Laboratory to 2.2 for the Rowett Institute. The three libraries in total subscribe to 1,862 primary journals and 121 abstract or other secondary journals, and each has a full-time library staff. Questionnaires of various sorts elicited replies from about 150 people, which represents about an 80% return of those approached.

It can be argued that the sample was small relative to the total number of biological scientists in Britain, or that it was in some way unrepresentative, so that the results lack generality. The concordance of results from the three institutes does not suggest this to be so. They nevertheless suffer from the defects of most surveys of opinion; what an individual believes, what he is willing to say he believes, and what may be inferred about his beliefs from analysis of his actions, differ. This has to be kept in mind in analyses which reflect motivation. The results are summarised in three parts, first those relating to the habits and views of workers as authors of primary papers, second their habits as readers and seekers of information, and third in terms of their underlying beliefs about the role of communication in science in the 1970s.

Working Biologists as Authors

An analysis of *curricula vitarum* of those workers who had published more than ten full papers showed that $76 \pm 14\%$ (s.d.) of their papers were published in three journals and that this was true even of the most prolific who had published more than 100. The remaining 24% were spread over many journals, each adding on average 0.6 of a journal to an author's list. This skewness of the distribution of journals and papers for individual workers is equally true of whole institutes. Figure 1 shows the total output of primary papers for each institute in six years against the number of journals in which these were published. It shows that most papers are published in a small proportion of the total journals used. Most surprising is that in all three institutes, catering for such a wide range of biological subjects, their 883 papers were published in only 168 journals. Furthermore, 76% of them could be accommodated in but thirty-four journals. Half the journals only occurred in the list because they had accepted one paper in the six-year period. These 'peripheral' journals were either very highly specialised ones, or journals competing with older established ones in a growing field and, from the dates of their first issues, were on average the younger journals.

This factual account of publishing habits at the three institutes can be equated with the views of individuals. Workers were asked to how many journals they could submit their current researches when complete. The results are shown in Fig. 2. They show the median number to be six. What these biologists chose and the reasons for choice are summarised in Table 1. Outstanding was the concern of the worker with the reputation of the journal within his field of study. Status is gained by publishing in such journals. The circulation of the journal—a matter relating to the universality of science—and delay in publication were not important considerations. Workers were not much interested in the format of journals though they were prepared to express forthright preferences for particular formats, methods of expressing and preparing references to previous work, standardisation of nomenclature and other editorial practices.

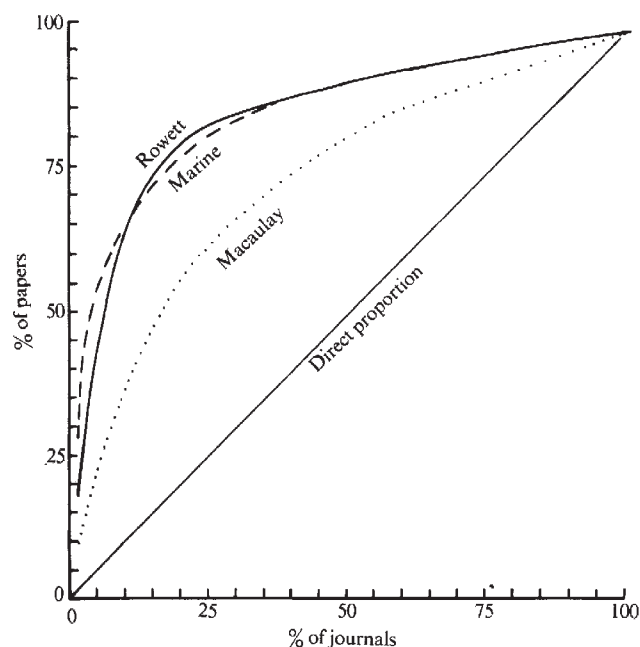


Fig. 1 Cumulative percentage frequency curves based on six years' publication of work by three institutes. The Rowett Institute published 423 primary papers in eighty-three journals, the Marine Laboratory 301 papers in fifty-seven journals and the Macaulay Institute 154 papers in fifty-two journals.

The wide choice of journal which workers state they have, and the very limited range of journals in which they in fact publish during their working lives, prompted us to ask whether they thought there were too many journals in their fields of interest. Most workers (63%) were not prepared to comment but of those who did 78% thought there were too many. Asked whether as a corollary there was not a case for amalgamation of journals, possibly on an international basis, more opinions were expressed; 34% were against and 25% in favour. The reasons given for rejection of the corollary were first that standards of journals might be threatened by amalgamation, and second that some work was of national rather than international scientific interest.

It would thus seem that individual biologists have a small number of journals in which they prefer to publish and to which they adhere during their working lives. They aspire to publish in journals with high reputations, and are not primarily concerned with the widest possible audience for their work, possibly thinking—and this was not examined—that secondary journals will bring their work to the notice of the whole community.

Table 1 Factors Stated to be Considered in Making a Choice of Journal in which to Publish Work by Workers in Three Research Organisations in Aberdeen

	Rowett Institute	Macaulay Institute	Marine Laboratory	All
The reputation and standards of the journal	85	85	84	85
The presence of papers on the same subject	78	85	90	83
The circulation of the journal	63	47	42	53
The delay between receipt of manuscript and its publication	34	43	35	38
The format of the journal	6	19	13	12
Number of respondents	68	47	31	146

Figures show percentage of total respondents who thought the factor important.

Working Biologists as Readers

The use of published material is of two types, first a fairly immediate one to identify new advances, and second a retrospective one undertaken to identify what has been accomplished in a particular field. The former is best called current awareness and the latter information retrieval.

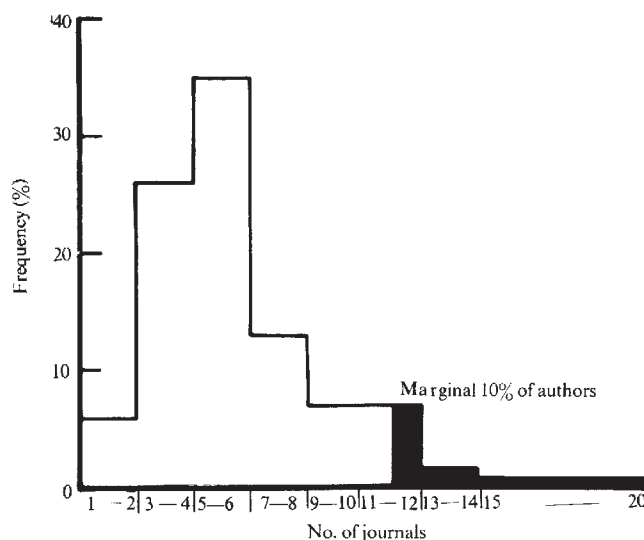


Fig. 2 Number of journals to which workers stated that the results of their current research work could be submitted. The data refer to all institutes and the median is six.

Using a variety of approaches it was quite clear that these biologists had no difficulty in keeping aware of current advances in their particular fields of research interest. Ninety-four per cent stated that they could keep up with current advances in their own fields and 39% even thought they could keep up with work within their own disciplines. Such a question in some ways predicates the answer, for to admit that one could not keep abreast of one's own speciality would imply an incompetence. Less open to interpretational difficulty were the answers to the question "How many primary journals should you read to give a 90% coverage of your field of research interest?". These are summarised in Fig. 3 from which it is evident that the median was twelve (eleven, twelve and twelve for the three institutes separately).

and that 90% of all respondents would gain 90% satisfaction if each was given funds to buy twenty-five journals.

The methods used by workers to keep aware of current work varied slightly, but not significantly, from institute to institute. Half the awareness came from a scanning of primary journals as they arrived in their libraries and a quarter from a scanning of review or abstract publications, a practice which accepts delay in the acquisition of information. The remaining quarter was accounted for by a scanning of lists of paper titles such as *Current Contents* or through colleagues drawing attention to particular papers.

Having, through a scanning process or in other ways, identified a paper relevant to his work, a worker usually wishes to ensure that he can find it again. The methods used to preserve current information were examined to show that 15% relied on their memory, 54% made a reference card or slip and 31% preserved the whole paper either by requesting a reprint of it or by making a photocopy. Photocopying was three times more important than requesting a reprint, indeed a variety of evidence suggests that reprint exchange as a method of scientific intercourse is in decline. Asked how far they relied on discussion and correspondence for dissemination of information, only 19% acknowledged that this method of communication was important. (See also Table 4 where a similar estimate of importance emerges from a different survey.)

It would thus seem that the individual's immediate needs for information are met by a small number of primary journals, a very small number indeed when judged by the hundreds scanned by abstracting journals. In addition, the scanning process is used to generate a highly personal retrieval system by most workers.



Fig. 3 Number of journals which workers stated that they should read to provide a 90% coverage of their field of research interest. All institutes, median is twelve.

Figure 4 shows the number of times workers stated they searched for information in the previous month. The median number was four times and 90% of workers searched less than ten times per month. Problems encountered in tracing what was published were not great for the Rowett and Macaulay Institutes but at the Marine Laboratory 42% of respondents acknowledged they had difficulty. It was evident that the major entry point in a literature search was not a perusal of the indexes of abstract journals or the use of computerised retrieval systems. Rather it was the references listed at the ends of primary papers or review articles. The entry points for the three institutes are shown in Fig. 5. Usage of abstract journals was checked at one institute. Consultations were less than 0.3 times per month per worker.

The workers were asked to recall on how many occasions they had referred to pre-war and nineteenth century literature.

Their replies suggested a very considerable reference to this older literature; 31% stated that they referred to nineteenth century literature. Analysis of library borrowings and inter-library loans, however, did not suggest that this older literature was consulted in depth, for only 4% of all borrowings and 11% of all inter-library loans were for material published

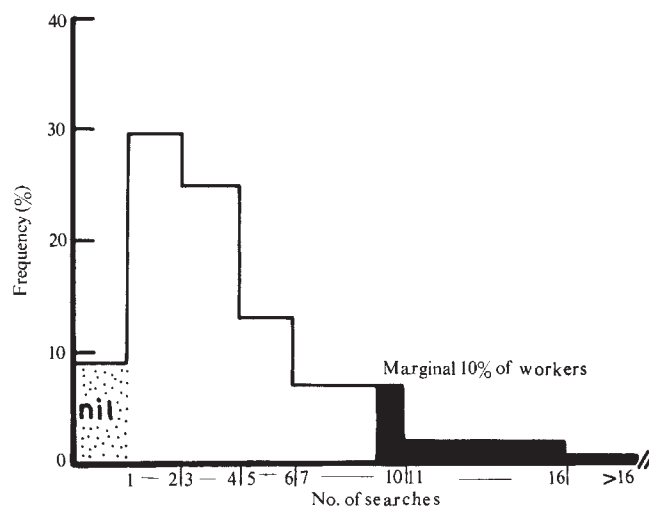


Fig. 4 Number of times workers stated they had searched for information in the previous month. All institutes, median is four.

before 1950. This anomaly is curious. Perhaps the question was equivocal; the term 'referred' could have been interpreted as 'considered' rather than 'perused', and the recollections do not necessarily imply that the paper or work was read in the original.

Library Policies

It has been implied that librarians are under pressure to purchase journals, and, being librarians, they like to preserve what they have purchased in a systematic way. They can never provide 100% satisfaction for all their readers from their own resources, and without inferring that they respond to pressures in any arbitrary way it can be suggested that their purchasing policy could be rationalised on the basis of the above analysis of opinion and habit. The argument is as follows.

If 76% of an individual's work is published in three journals irrespective of his prolificacy and if 76% of an institute's work over a contemporary period is published in fifteen, and if the shapes of the marginal distributions are similar, then that institute covers five 'fields'. Fields so defined are in terms of journals and the definition is not independent of the interpretation by journal editors of what constitutes their subject matter. Then, if 90% of the workers obtain 90% coverage of their current research interest from twenty-five journals, the number of journals to be taken by an institute with five fields is $5 \times 25 = 125$. This number should give 90% satisfaction to 90% of its users and a library so defined is a working library. It should have a working life and it can be argued from the pattern of borrowings and loans that this should be 25 years, for direct reference to work published a quarter of a century earlier is less than 5% of total reference. The precise cut-off obviously depends on the cost of shelf space relative to the cost of book postage, it being assumed that a network of archival libraries exists and that efficient and legally trouble-free methods of photocopying are in operation.

The libraries of the three institutes differed. Two, those of the Rowett Institute and Marine Laboratory, cater for

more than the immediate needs of their staffs and are regarded as major libraries in the sense that they are United Kingdom repositories for information in nutritional sciences and marine sciences respectively. The library of the Macaulay Institute does not have such a wide additional commitment. The Rowett library and Marine library are seven and eleven times the size they should, in theory, be were they working libraries. The Macaulay library of 290 primary journals, however, is not much greater than the size inferred from these theoretical

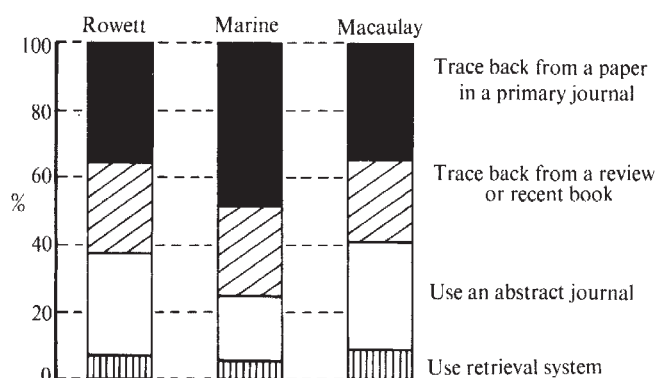


Fig. 5 The methods stated to be used to trace work already done in a particular field by workers in three institutes.

considerations—200 journals. There was no evidence that the workers at the Macaulay Institute had any greater problems in keeping aware of current work or in information retrieval than did workers at institutes housing the two larger libraries. This suggests that the arguments about optimal size of library are correct.

Beliefs about Scientific Communication

Communication of research findings through publication in a journal with a referee system is central to the whole social system of science. It is the way in which the individual relates to others in the scientific community. Traditionally and ideally it is held that the individual scientist makes his work known through publication and receives in return approbation and esteem among his fellows. The exchange is free and the judgment is by equals bound by no contractual relationship. The exchange is not simply here and now but international and through time; it is also basically disinterested and the participants non-competitive. Merton⁶ has stated "The substantive findings of science . . . constitute a common heritage in which the equity of an individual is severely limited."

This traditional ideal if it ever existed in an unalloyed state—is subject to erosion and change, because of the growth of science, its increasing complexity and its institutionalisation. The intrinsic control processes in which communication is the main feature are more and more in conflict with external ones. Scientists are also employees, while government and industry finance research with particular ends in view. Furthermore, the media of the exchange process—the primary scientific journals—are under commercial pressures and a growing proportion are under commercial control.

It has frequently been suggested^{7,8} that these problems, combined with the pressures caused by growth and fragmentation of science as a whole, are destroying the basic value system on which the scientific activity is based; in other words that the 'breakdown' of the publication system, if indeed a breakdown exists, not only illustrates the practical problems of 'big science', but also involves a fundamental change in the motivations and belief systems of

scientific workers. It therefore seemed of interest to try to find out to what values the workers at these three institutes were willing to subscribe. Did they still uphold absolutely the 'ideal' values, despite the practical problems? Did they, at the other extreme, take a defeatist view and suggest that these values no longer had any relevance? Or did they find some compromise, easy or uneasy?

The workers were asked questions designed to represent this spectrum. They were offered a list of statements and asked which represented their view most nearly; if necessary they could (as a form of compromise) choose more than one. Questions were designed to explore beliefs about the system of publication. Briefly, they ask first: is the ideal of a universality of knowledge practicable?; second: is the 'reward' system in fact operating equitably?; and third: is the scientist's responsibility to participate in the system still rigorous, despite the problems of scale?

Table 2 Classification of Replies to the Question: When you Write a Paper, for what Potential Audience do you Feel that you are Writing?

	Percentage of respondents		All
	Age 39 or below	Age 40 or above	
The whole scientific community	14	9	12
The members of one discipline (for example, chemists, physiologists)	42	51	46
The members of one sub-discipline (for example, lipid chemists, rumen microbiologists)	35	32	34
A small number of interested people most of whom you could name	9	6	8
No audience—writing papers is only a ritual activity	3	5	4
Number of respondents	88	65	153

It has been suggested⁵ that the traditional methods of communication and control can be maintained only if new groupings, new subdisciplines, provide an audience of practicable size. The workers were therefore asked whether they still held the ideal value that work is addressed to the whole scientific community, or whether they saw themselves as focusing on smaller groups. Their replies are given in Table 2 where they are classified by age; in the surveys of opinion which follow differences between institutes were minor. It was perhaps a little surprising that, despite the trend towards the narrowing of specialties, nearly half of these workers claimed to be writing for the members of a whole discipline, and a substantial proportion thought that they were addressing 'the whole scientific community'. The younger workers, perhaps because they were less distant from the socialising processes of scientific education, were rather more inclined to envisage the traditionally wide audience. In general, these answers would seem to suggest a conflict between the traditional values of scientific publication and the practicalities of the current situation.

A second question concerned the efficient and impartial working of the reward system. The replies are given in Table 3. Again, these answers might be thought to show some conflict, and some doubt in these workers' minds about the impartiality of the system and the traditional value of individuality. Few thought that 'rewards' were influenced by organisational status, but a third thought that the reputation of the laboratory was a factor, and only about a fifth, overall, thought that credit was given wholly impartially.

Evidence of disillusion came also from answers to a

question designed to assess the apportionment of credit for work reported in a multi-author paper. Two-thirds said that they used their personal knowledge while names on papers were thought by 7% of the workers to have a decreasing relevance nowadays. This perhaps suggests a concern about the operation of the traditional system of reward.

Table 3 Replies to the Question: Which of the Following would you say is Most True at Present?

	Percentage of respondents		All
	Age 39 or below	Age 40 or above	
Scientific work is invariably judged on its merits, irrespective of who the author is	20	25	22
Reaction to work is influenced by the reputation of the laboratory or organisation in which it is done	39	31	36
Reaction to work is influenced by the author's scientific reputation	54	55	54
Reaction to work is influenced by the author's status in his organisation	7	4	6
A young and unknown worker would have difficulty in getting unconventional work accepted, unless well sponsored	22	19	21
Number of respondents	87	68	155

The purpose of a third question was to find out what these workers thought of the scientist's obligation. Did they wish to uphold the rigorous traditional view that their first duty was to share their work? Or, in the face of the current difficulties, did they see their obligation as merely to 'keep up' in a general or a specialised way?

Table 4 Replies to the Question: Which of the Following best describes the Obligation of the Scientist with regard to Communication?

	Percentage of respondents		All
	Age 39 or below	Age 40 or above	
He has an obligation to see that others in his field know what he is doing	34	26	32
He has an obligation to see that he knows what is being done in his field	75	59	66
He ought to keep up with the key journals in his subject	30	37	33
He ought to maintain personal contact with others working in his field	18	12	16
It is impossible to keep up with the literature nowadays	7	4	5
Number of respondents	87	68	155

Few people were willing to say—even anonymously—that they thought the system of communication had broken down completely. Their views of their obligations were on the whole rigorous, whether or not it was practicable for them to fulfil them. The younger scientists seemed

to have a rather more idealistic view of the possibility of 'keeping up' in their field.

Implications

Though on a small scale, though confined to biologists with no educational commitment and though, like many investigations designed to ascertain belief, plagued by uncertainty, the investigations provide evidence that the scientific community can cope with a so-called information explosion which apparently causes consternation to librarians, archivists and information scientists. Individuals use very few journals; they actually publish in a small number and they do not contemplate doing otherwise; they identify a number of key journals to read which is a minute fraction of the many thousands which could be regarded as relevant to their fields of research. The individual thus copes by narrowing the overall information problem to a practical size. Even on the scale of whole research institutes there is evidence that the information problem is not immense, for large groupings can be inferred to take place.

These behavioural patterns imply that some of the classical ideals of science no longer apply and that the universality, communality and disinterestedness of science have been tempered by practical expediency. Yet many workers still subscribe fully to these ideals and are reluctant to admit that times have changed. In correspondence some castigated practices of multiple publication, the splitting of work into separate parts and the publication of short papers rather than complete accounts of work, adducing reasons why quantity of publication appeared to them to be more important than quality as far as individual advancement in a career sense was concerned. Certainly, besides the conflicts between practical possibility in information transfer and the ideals of the universality of knowledge there are additional conflicts implicit in publication which relate to the professional and scientific status of a worker and the financial reward which he receives. The old saw 'publish or perish' has financial overtones as well as those of approbation.

It has to be recognised that there are limits to the capacity of individuals to assimilate information and that each individual research worker studies within a limited range. It also has to be recognised that journals are articles of commerce subject to laws of supply and more particularly of demand. It can be envisaged that journals will continue to proliferate and divide to cater for these limited demands of workers but that new economic difficulties will emerge as a result. Equally proliferation raises a series of problems about the achievement of high standards in terms of acceptance policies within such journals, for not only is the quality of a journal important to the individual who publishes therein, but it also relates to the whole value system of science which quite clearly individuals are unwilling to jettison. These problems are some of the more pressing ones that the scientific community has to face at the present time.

We thank those who responded to our various questionnaires and who corresponded with us. Mrs A. MacDonald helped in the collation of the results.

¹ Stevens, P. C., *Libr. Ass. Rec.*, 74, 8 (1972).

² *Annual Abstract of Statistics and Monthly Digest* (HMSO, 1972).

³ Hills, J., *Aslib Occasional Publication No. 9* (1972).

⁴ Price, D. J. de S., *Little Science, Big Science* (Columbia University Press, New York, 1963).

⁵ Price, D. J. de S., in *Factors in the Transfer of Technology* (MIT Press, 1969).

⁶ Merton, R. K., *Social Theory and Social Structure* (Free Press, New York, 1957).

⁷ Hagstrom, W. O., *The Scientific Community* (Basic Books, New York, 1965).

⁸ Rose, H., and Rose, S., *Science and Society* (Penguin Books, Harmondsworth, Middlesex, 1970).

Search for Short Bursts of Gravitational Radiation

R. W. P. DREVER, J. HOUGH, R. BLAND & G. W. LESSNOFF

Department of Natural Philosophy, The University, Glasgow

Recent data from an experiment in Glasgow suggest that pulses of gravitational radiation of less than a few milliseconds duration are too infrequent to account for signals reported by Weber in 1970.

WEBER'S experiments^{1,2} have for some years suggested the presence of unexpectedly large bursts of gravitational radiation coming from outside our Solar System. The bursts last less than 0.5 s, and one or more is detected per day. Here we report the development and operation of detectors designed to give much more precise and detailed information than could be obtained with the original Weber detectors and on observations with these instruments.

Our detectors consist basically of two separate masses linked by piezoelectric transducers to monitor changes in separation, an arrangement equivalent in some ways to the Weber bar but giving much larger coupling between the mechanical and electrical systems. This results in a relatively large electrical output, so that it becomes practicable to use amplifiers of wide bandwidth and to record the motion of the masses with a resolution of the order of a millisecond. (A similar arrangement was originally proposed by P. Aplin, University of Bristol.) If one considers an idealised detector with point masses and a weightless transducer, the apparent force acting on each mass can be deduced from the strain in the transducer. In fact a simple analogue circuit can be used to derive the effective force induced by a gravitational wave from the electrical output of the transducer. This filter circuit has a null at the resonant frequency of the mechanical system, since the resonance corresponds to motion without external forces. With a filter which has a response which is the inverse of that of the mechanical system the whole arrangement becomes effectively non-resonant.

Gravitational radiation detectors of this general type have been analysed theoretically from different points of view by Gibbons and Hawking³ and Buckingham and Faulkner⁴, who have shown that for a short pulse input, the output signal-to-noise ratio is proportional to $(\beta Q)^{\frac{1}{2}}$, where β is the coupling constant between the mechanical and electrical systems, and Q is the quality factor of the mechanical resonance. This signal-to-noise ratio is not the only factor to be considered in assessing the sensitivity of the detector in an actual experiment, but it is an important one, and large values for this factor have been achieved in our detectors. The wide bandwidth, good time resolution, and high sensitivity of these detectors give one the possibility of investigating suspected gravitational radiation signals in

a much more detailed and critical way than is possible with other currently available detectors.

Detector Arrangement

A schematic diagram of one of our detectors is shown in Fig. 1. The two aluminium half-bars are bonded to six stacks of piezoelectric transducers, and each half-bar is suspended at its centre of gravity from an aluminium support structure which is itself supported at its centre by a bridge. This bridge is a flat aluminium plate resting on stacks of lead blocks separated by pieces of rubber which provide the main isolation from vibrations. The whole detector assembly sits on a trolley which rolls into the vacuum tank on rails. Each detector has a mass of 300 kg and a total length of 155 cm.

The signal from the transducers passes to a field-effect transistor amplifier and then through the filters F1 and F2. The filter F1 is a bandpass filter used to define the bandwidth of the system; the band transmitted is approximately from 650 Hz to 1,450 Hz, centred near the fundamental frequency of the detector. The filter F2 is used to extract the signal from the resonant motion of the detector. In some of our experiments an analogue computer circuit which solved the equation of motion of an idealised detector was used; its frequency response is shown in Fig. 2a. With this filter the complete system reproduces the main features of the input force waveform, but the rapid increase in gain at high frequencies does increase the noise level of the system. If the waveform information is of less importance than the signal-to-noise ratio, a filter with a narrower notch is useful. The simple LCR filter shown in Fig. 3, with the response of Fig. 2b, is convenient. With this filter, the measured performance of our system for bipolar pulses was close to that predicted for the optimised filtering system of Buckingham and Faulkner⁴.

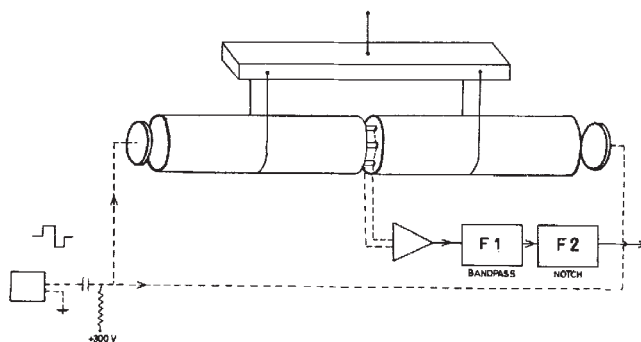


Fig. 1 Schematic diagram of one of the detectors, including the arrangements used for applying calibration forces to the bars.

Calibration and Sensitivity

The system was calibrated by applying forces of known magnitude and waveform to the ends of the detector by the electrostatic attraction of a pair of fixed electrodes. The signals were superimposed on a fixed polarising voltage, as shown in Fig. 1. The steady force applied to the detector in this arrangement is ignored by the electronic system, and the effective force change is closely proportional to the alternating test voltage waveform. Calibration was done with bipolar pulses of total duration equal to the detector period. For one of our detectors, $\beta=0.18$, $Q=2,200$, the resonant frequency is about 1,100 Hz and the mean output noise corresponds to about $0.015kT$. The other detector, the first which we built, had slightly poorer performance with $\beta=0.05$, $Q=1,400$, resonant frequency of about 1,020 Hz and a mean output noise corresponding to $0.033kT$.

The sensitivity of our experiment may be expressed in terms of energy thresholds, which we define operationally as follows. In the case of a single detector test force pulses are applied, and the amplitude of the pulses is adjusted so that the output of the notch filter triggers a threshold discriminator for 50% of the pulses. Then the energy which such a pulse would feed into the first longitudinal mode of a detector which was initially at rest—that is a bar system with no thermal excitation—is calculated and expressed in terms of the thermal energy kT of a normal mode of the real detector. This energy we define as the '50% threshold' of the single detector. This idea may be extended to a two-detector coincidence experiment, where it is convenient to define a '25% threshold' at which coincident test pulses of equal equivalent energy are detected by the coincidence system with an efficiency of 25%. It may be noted that if the two detectors are not of equal sensitivity this threshold will not correspond to the 50% efficiency point of each detector individually.

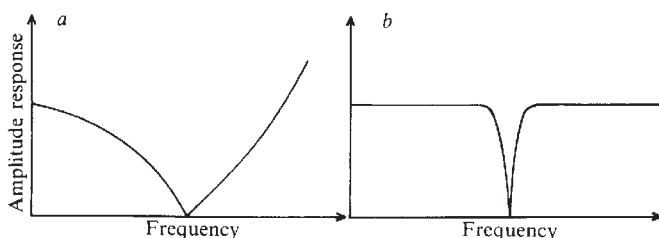


Fig. 2 Frequency response of an inverse filter for an idealised detector (a) and of a simple LCR filter (b).

In our present experiments the thresholds varied somewhat among the different recording techniques used. For one detector the 50% threshold lay in the range from 0.12 to $0.15kT$ for the main recording systems. The corresponding 50% threshold for the other detector varied between 0.2 and $0.5kT$ during the period of the experiments.

The experiments described here are most sensitive for short pulses of duration 3 ms or less, which might be expected from gravitational collapse, and so are not such sensitive tests for longer pulses which might be detected by Weber's equipment.

Preliminary Experiments

It was suggested some years ago by Weber¹, and more recently by Bertotti (private communication), that the forces caused by gravitational waves in the bar of a detector might release stored energy, so that a magnified signal might be obtained. We have therefore carried out experiments to try

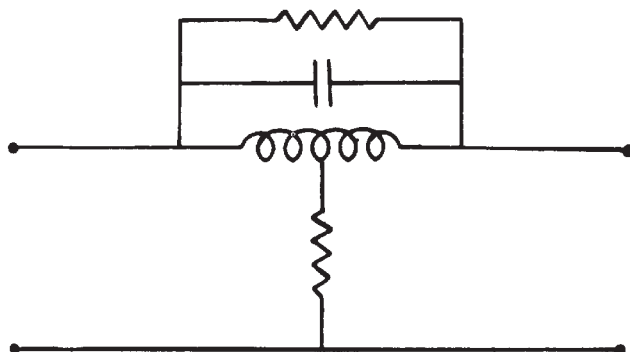


Fig. 3 LCR notch filter used to extract changes in the amplitude and phase of the resonant motion of the detector.

to observe such a process. In one test we applied impulsive forces to one of our detectors, of an amplitude just below our detection level, and at a rate of 8.64 d^{-1} . The time of occurrence of each was indicated by a subsequent train of large pulses which were easily seen. In a run lasting about two months, none of the small pulses became detectable. Another test was made using a train of five equal amplitude force pulses (corresponding to a mean energy change of several times kT) separated by intervals of 20 s. The train of pulses was repeated 8.64 times per day. One might have expected any amplification process to be more effective for the first pulse of a train than for the last, if the recovery of stored energy in the bar is a slow process and the first pulses in a train have depleted the accessible energy. We in fact see no significant change in observed pulse height. From these results and also from the agreement of our calibration measurements with calculated predictions, we conclude that there is no important multiplication process in this detector. More details of this and other experiments with a single detector have been presented elsewhere^{5,6}. (Some of the results presented now differ slightly from those given in ref. 6, as a result of more extensive and precise evaluation of the data, and incorporation of additional observations.)

Coincidence Experiments

For these observations the two detectors were set up 50 m apart, with their axes north-south. (The latitude is 56° N .)

A simplified schematic diagram of the arrangement used for data recording throughout the present experiment is shown in Fig. 4. The chart recorder and the time delay analyser use only the notch filter output. The coincidence system is triggered from the notch filter output, but other signals, some of them seen before the notch filter, are recorded on the photographs.

The chart recorder monitors the notch filter output from the two detectors as well as other signals for test purposes. As our wideband detectors can give output signals of duration a few milliseconds or less a pulse-stretching technique is used with the chart recorder to ensure that the pens do not miss any pulses.

For highest sensitivity, a recording device faster than the chart recorders must be used. We have therefore developed a system for photographic recording of data displayed on an oscilloscope, triggered by electronic coincidence circuits. The signals from one detector are delayed by 0.8 s in an f.m. tape recorder and the same threshold discriminators and coincidence circuits look concurrently for both prompt and delayed coincidences between the two detectors. The photographs of prompt and delayed coincidences look similar, and are marked in a way which is not apparent to the person scanning the records until after the photograph has

been measured, thus minimising the effect of human bias in the analysis. The signals pass through digital delay units before the oscilloscope to enable some of the signal prior to the coincidence to be recorded. A total time interval of 20 ms is displayed on the oscilloscope.

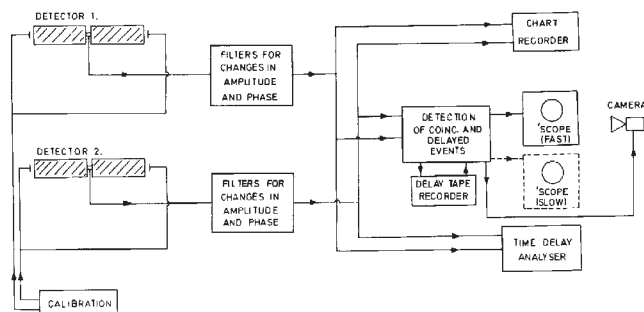


Fig. 4 Simplified block diagram of the data recording system.

A third method of recording data involves a system which measures time differences between nearly coincident pulses from the two detectors, using a 400-channel pulse height analyser operating in multiscaler mode. A pulse from one detector causes the analyser to sweep through its channels at a fixed rate. If a pulse occurs in the other detector, it is delayed by exactly half the total sweep time and then the number of counts in the channel at the sweep position is incremented. Thus at the end of a run the analyser automatically produces a time delay distribution.

Experimental Results

A result of limited accuracy can be obtained from the chart recordings alone. A short stretch of chart record (32 d) taken during September and October 1972 was analysed by scanning the trace for each detector separately and then searching the lists of pulse times for coincidences. The chance coincidence rate was determined by searching for delayed coincidences with displacements of 1, 2 and 3 h in the pulse times for either channel. Taking a coincidence window of 18 s and a two-detector 25% threshold of $0.6kT$ gave a coincidence rate above chance of 2.3 ± 2.1 per month of 28 d.

Many more precise and extensive data have been obtained using the electronic analysis systems.

The time delay distribution from the multichannel time analyser is shown in Fig. 5, for data taken over a period of 7 months, from September 1972 to April 1973. The upper plot covers delays from -2 to $+2$ s; the lower plot shows the central region on an expanded scale. There is no significant excess of pulses in the central region of the distributions, whereas tests showed that almost all short coincident pulses would have been recorded in the two central 10 ms bins. In those two channels we find an excess of 5 ± 11 counts in 150 d of running, corresponding to a rate of coincident pulses above the chance rate of 0.9 ± 2.1 month $^{-1}$ at a mean two-detector 25% threshold of $0.30kT$.

We now discuss data from the photographic recording system, over the same period of 7 months. The oscilloscope was triggered by pulses from the two detectors, above the discriminator thresholds, arriving within 6 ms of one another. The photographs were scanned and measured, and the pulse heights and time delay for each event were punched on to computer cards for subsequent analysis. Both prompt coincidences and coincidences with one signal delayed by 800 ms were analysed.

Analysis of the time delay distributions obtained from the

photographic records shows no significant peak at zero delay. Tests with artificial pulses near threshold indicate that about 85% of short coincident pulses would be recorded within a 1.6 ms time interval around zero delay, and we find that the numbers of events in the prompt and delayed distributions within this time interval are not significantly different. The number of coincident events above chance determined in this way is -1 ± 28 in 180 d, or -0.16 ± 4.4 per month. The mean two-detector 25% threshold for this result is about $0.27kT$, taking account of the 15% loss of coincident pulses which fall outside the central 1.6 ms time interval.

The pulse height information from the photographic records is also of interest, particularly in searching for rare coincident events. In Fig. 6a and b we show two-parameter plots of pulse heights from the two detectors for prompt and delayed coincidences. A short pulse of gravitational radiation would be expected to give a prompt coincidence with roughly equal pulse heights in the two detectors. Dotted lines are drawn at equivalent energies of $0.5kT$ for the two detectors, and most coincidences are seen to have energies less than this value in both detectors; a smaller number have energies greater than this value in one detector. There are two prompt coincidences and one delayed coincidence with energies greater than $0.5kT$ in both detectors. The delayed coincidence and one of the prompt ones are only approximately in coincidence, having time differences between the pulses on the two detectors of 3.8 and 5.3 ms, respectively. The remaining prompt coincidence, however, has a time difference of less than 0.5 ms. This is our best candidate for a true gravitational radiation event. It registered equivalent energies of 0.64 and $0.61kT$ on detectors 1 and 2, respectively, and was recorded on September 5, 1972, at 13 h 07 min 29 s UT.

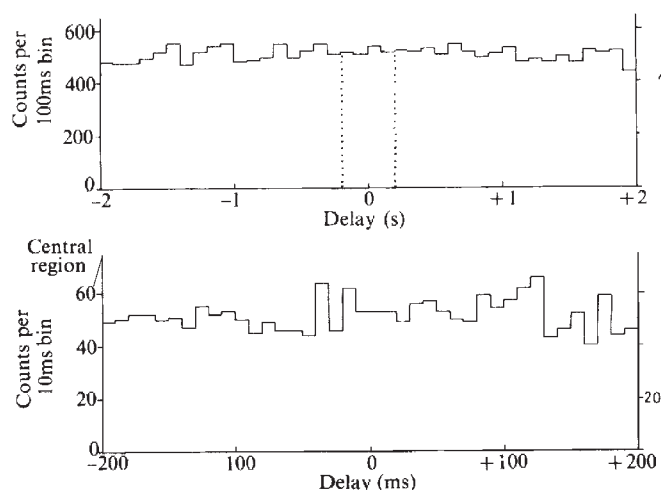


Fig. 5 Distribution of time delays between pulses from detector 1 and detector 2 for 7 months of observation (September 1972–April 1973). The 400 channels of the time analyser are grouped in tens in the upper plot, and the individual 10 ms channels for the central 400 ms (dotted lines in upper plot) are shown in the lower plot.

The photographic records contain a considerable amount of detailed information about the coincidence events, including the thermal vibration of the detectors, for 10 ms before and after the time of the coincidence. The photographic record for the event of September 5 is shown in Fig. 7. Here the top and bottom traces are the signals after bandpass filters F1, and show the thermally excited oscillations of the two detectors. The two central traces

are the same signals after the notch filters and indicate the forces acting on the detectors. (These signals do not represent the waveform of the force acting on the bar, however, since the filter used here was not a true inverse filter.) Important points to note are: (1) the signals on the central traces are in phase; (2) the signals on the central traces are similar in amplitude, waveform and duration, and correspond to a pulse of equivalent energy of about $0.63kT$, with a duration of less than 3 ms; (3) there is an evident change of amplitude of the top trace, and a less evident but equally significant change of phase of the bottom trace. Each of these changes is consistent with the corresponding signal from the notch filter. This shows that the signals arise from forces applied to the bars rather than from electrical interference in the amplifier or elsewhere.

To establish the significance of this event, we must esti-

mate the probability of obtaining a signal of this general character by chance. We have done this using the measured spectra and counting rates from each of the detectors near the time when the event was observed, and evaluating the rate expected for coincident pulses of amplitude equal to or greater than 0.8 of the amplitude of the signal seen on each of the centre traces of Fig. 7 and occurring within

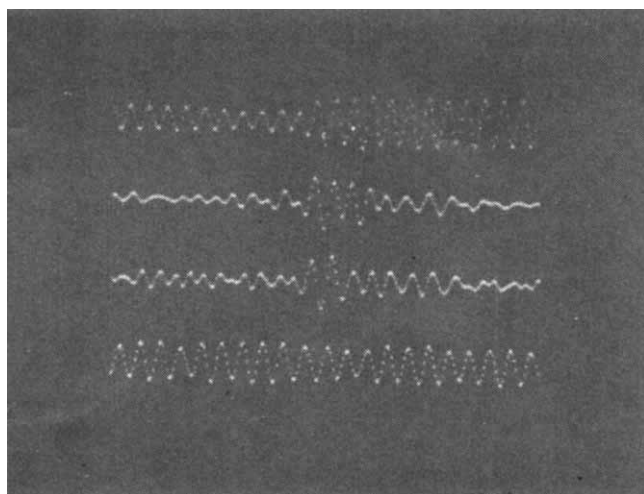


Fig. 7 Photograph of the event recorded on September 5, 1972, at 13 h 07 min 29 s UT.

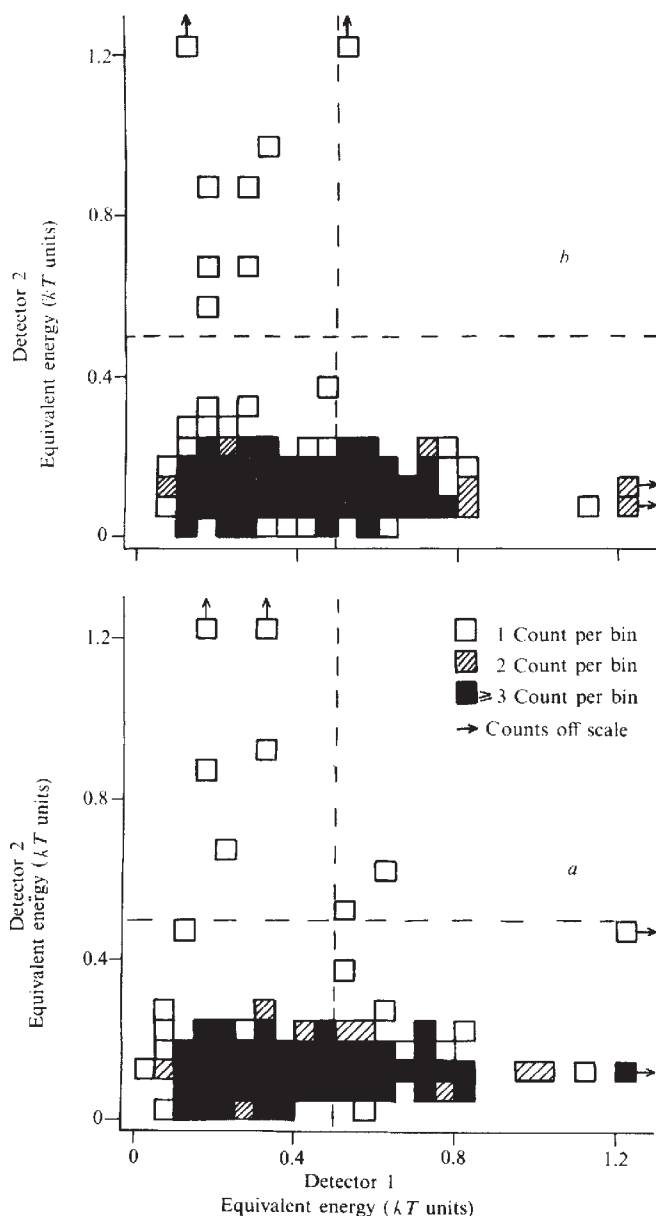


Fig. 6 Plots of equivalent pulse energies from the two detectors, for all coincident events within the 6 ms resolution time, for *a*, prompt coincidences; *b*, delayed coincidences. Data from September 1972 to April 1973. We note that there were slight changes in the noise levels of the detectors during the course of the experiment, altering the distribution of the pulses to some extent.

0.5 ms of one another. We find that one might expect one coincidence satisfying these criteria per 100 yr. Since these criteria are rather loose, it seems very improbable that the observed event is a chance coincidence of unrelated phenomena in the two detectors. At present we know of no experimental reason to reject the hypothesis that this signal was caused by gravitational radiation.

Comparison with Weber's Experiments

An unsuccessful search for gravitational radiation was reported about a year ago by Braginskii *et al.*⁷, with detectors of comparable sensitivity to those of Weber. Very recently, while the present paper was being prepared, null searches with single detectors have been reported by Tyson⁸, using a large and sensitive detector having, however, a pass band which did not overlap any of those of Weber's experiments, and by Garwin and Levine⁹ with a smaller detector of lower sensitivity.

Our present experiments were not designed primarily to confirm or refute Weber's observations, but the facts that our relatively wide bandpass does overlap one of Weber's experiments at 1,030 Hz (ref. 10) (where he observes the same number of coincidences as at 1,660 Hz) and that our apparatus is relatively sensitive, make it interesting to compare our observations with Weber's findings. This comparison is valid, however, only under the assumption that Weber's pulses are of short duration (a few milliseconds or less).

The signal analysis methods used in most of Weber's experiments and the lack of published data on energy calibration and pulse detection efficiency make it difficult to estimate precisely the energies of the gravitational radiation bursts which could be responsible for the signals he observed and the rate of bursts implied. However, it is possible to compare our experiment with one of Weber's in a simpler way. We take a particular energy of gravitational radiation pulse, and calculate an upper limit to the pulse detection efficiency

of Weber's experiment as determined by fluctuations in the thermal energy in the bar alone, for this size of burst. If we assume that the coincident signals reported by Weber were due entirely to gravitational waves of this energy, we can deduce a lower limit to the rate of occurrence of such bursts. Repeating the calculation for a range of energies, we can obtain a lower limit to burst rate as a function of possible burst energy. In our own experiments we have directly measured the detection efficiency for a wide range of applied force pulses, so for each assumed gravitational wave burst energy we can derive a rate of occurrence of bursts from our observed coincidence rate, and this can be directly compared with the corresponding lower limit to burst rate deduced from Weber's observations.

Weber has used various methods of analysis in his different experiments, but one set of data which is particularly amenable to this comparison is that obtained in the period May 20 to November 20, 1970, and presented graphically in Fig. 3b of ref. 2. The events plotted there were selected from the noise fluctuations registered on chart recorder tracings on the basis of two main requirements: (1) a coincidence circuit had indicated that the signals from the two detectors had crossed a certain threshold within 0.5 s of one another, and (2) the energy of each bar had exceeded another threshold for each bar, this threshold being defined by a condition that there be 100 crossings of the threshold per day. Either of these requirements could be used alone to set an upper limit to possible efficiency; we will use requirement (2). If we assume that the bar has mean thermal energy kT in the mode being studied, and that the quality factor of the cylinder is 70,000, then it can be deduced that the energy of the mode will exceed a threshold of $4.8kT$ 100 times per day. Additional noise from amplifiers or transducers can only increase the rate of crossing any given threshold, so we can take $4.8kT$ as a lower limit to the effective thresholds in this experiment.

An incident gravitational wave may either increase or decrease the energy of the cylinder vibration, and the sign and magnitude of the change will depend on the phase relationship between the gravitational wave and the bar, and the initial energy of the bar. For short gravitational wave bursts of given equivalent energy, the probability that the energy of the cylinder will exceed a threshold of $4.8kT$ within one bar damping time of the pulse can be calculated¹¹. For a simple threshold-crossing detection method we might take this probability as being a first approximation to the probability of detection of these gravitational pulses, or the "detection efficiency" of the detector. We find, for gravitational pulses of energy $3kT$, a detection efficiency of 0.3, for kT , a detection efficiency of 0.075, and for $0.3kT$, a detection efficiency of 0.020 for one of Weber's detectors. In a coincidence experiment a given gravity wave burst must be detected in both detectors so the efficiency of coincident detection cannot be higher than $(0.3)^2 = 0.09$, $(0.075)^2 = 0.0056$, and $(0.020)^2 = 0.00040$, respectively. The actual efficiency will be less than these values in practice, for (1) the presence of amplifier and transducer noise will decrease the efficiency for a given threshold crossing rate, and (2) the additional requirement of operating a coincidence circuit of relatively short resolving time cannot increase the detection efficiency, and may decrease it significantly.

The rate of observed coincidences above chance reported by Weber in the data under consideration is $0.75 \pm 0.07 \text{ d}^{-1}$. Taking the upper limits to coincidence efficiency just given, we find for gravitational waves of energy $3kT$ that the minimum incident rate of bursts is 7.5 d^{-1} , for kT it is 133 d^{-1} , and for $0.3kT$ it is $1,880 \text{ d}^{-1}$.

For our own present experiment, the coincidence efficiency for the two central 10 ms bins in the time analyser data was found by measurement to be 0.8 at $3kT$, 0.055 at kT , and 0.00021 at $0.3kT$. (The energies are those given to Weber's bar by the pulse in question.) Our observed coincidence

rate above chance is $0.03 \pm 0.07 \text{ coincidences d}^{-1}$. Thus our experiment gives an incident rate for gravitational wave bursts of $0.04 \pm 0.09 \text{ d}^{-1}$ for $3kT$, $0.6 \pm 1.3 \text{ d}^{-1}$ for kT , and $150 \pm 340 \text{ d}^{-1}$ for $0.3kT$.

All of our rates here are significantly smaller than the minimum corresponding rates of bursts implied by Weber's data, on the assumption that the sources of Weber's coincident signals are millisecond pulses of gravitational radiation. This simplified analysis does not itself show a contradiction between our results and Weber's provided that the pulses seen by Weber had energies of $0.25kT$ or less, and a corresponding rate of occurrence of 2,300 or more per day. A more realistic analysis would give an even lower efficiency for Weber's experiment, would require a larger incident rate and would extend the region of apparent discrepancy to still smaller pulses. If Weber's signals were due to pulses smaller than these, the total energy flux implied would be even larger than previous estimates have suggested.

Overall Implications

We have presented the results of seven months of operation of two wide-band gravitational radiation detectors. In this time we have observed one distinctive signal which fulfils the requirements expected of an event due to a short pulse of gravitational radiation, at a level which would give an energy of $0.63kT$ to each of our detectors, initially at rest.

In 1970 Weber reported detecting a flux of about 1 event d^{-1} (ref. 2). We have attempted to assess the performance of Weber's equipment in a way which gives an upper limit to its sensitivity, and have made a comparison with our experimental results. We conclude that it is unlikely that the signals reported by Weber in 1970 were due to pulses of gravitational radiation of duration less than a few milliseconds, assuming that the source has not changed significantly since then. But our present observations do not exclude the possibility that Weber may have detected bursts of gravitational radiation of much longer duration.

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LETTERS TO NATURE

PHYSICAL SCIENCES

Laser Radar Observations of Atmospheric Potassium

LASER radar measurements of night-time atmospheric sodium are now an established technique; they have already made significant contributions to our knowledge of the distribution of sodium atoms in the 75 to 105 km height region¹⁻⁶, and have recently been extended into daytime⁷. It has often been the practice in airglow methods to study sodium, potassium and lithium simultaneously since their chemical and physical properties are similar, but, to date, no laser radar measurements of either lithium or potassium have been reported. We report here the result of an experiment to detect and measure the density and distribution of atomic potassium in the 76 to 100 km height region using a laser radar system.

A tuned dye laser was used to generate a resonance wavelength of potassium, 769.9 nm. The lasing medium was a solution of 3,3'-dimethyl-2,2'-oxatricarbocyanine iodide in ethylene glycol⁸, which was circulated through a cylindrical cell 50 mm long and 10 mm diameter and pumped, nearly longitudinally⁹, by a pulse from a Q-switched ruby laser. The tuning elements were two tilted Fabry-Pérot etalons¹⁰ and a tuned solid etalon of 47% maximum reflectivity which was used as an output reflector. The laser spectrum was recorded on infrared plates using a grating spectrograph. The laser gave an output of about 40 mJ with a linewidth of the order of 10 pm. Because the column density of potassium was expected to be one hundredth of that of sodium and because the laser output and photomultiplier efficiency were lower than in equipment commonly used to study sodium, it was expected that the signal would be comparatively weak. It was therefore decided to use the large receiver, 15 m² collecting area, of the mark II laser radar system at Kingston, Jamaica, described by Kent, Sandland and Wright¹¹, and the dye laser was incorporated into this system.

In Table 1 we summarise the results of 600 laser firings. The photons were counted from altitudes between 31.5 and 35.5 km where Rayleigh scattering by the air would be expected to be dominant, between 75.5 and 99.5 km where resonance scattering by potassium should occur and Rayleigh scattering should be negligible, and between 103.5 and 151.5

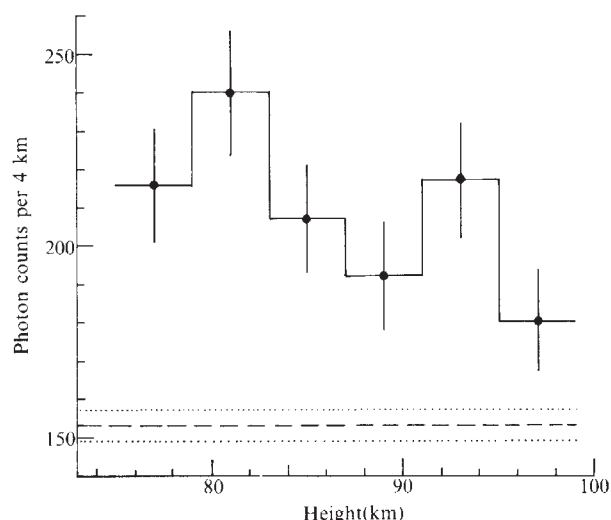


Fig. 1 Postassium return on January 25 and 27, 1973. The points are the scattered signal and background noise summed over 4 km height intervals. ± 1 s.d. of the count is indicated by the vertical error bar on each point. ---, Average background noise determined from measurement at heights of 103.5 to 151.5 km; . . . , ± 1 s.d. of the background noise.

km where the counts should be a measure of the background noise. These noise counts have to be subtracted from the counts at lower altitudes to determine the signal. A highly significant signal of 334 ± 45 counts was obtained from 75.5 to 99.5 km. To prove that the signal was not due to a spurious source of noise, the laser was detuned and the result (Table 1) showed that the counts in the 75.5 to 99.5 km region were not significantly different from the measured noise. The data in Table 1 also show that the Rayleigh scattered signals were not the same in the two cases, due to a loss of output when the laser was detuned. Nevertheless, the measurement of January 27 gave at 75.5 to 99.5 km an excess of 0.117 ± 0.020 counts per Rayleigh count; when detuned a significantly smaller excess was obtained, -0.007 ± 0.047 counts per Rayleigh count. We conclude that resonance scattering from potassium was observed.

The effective total scattering cross section of a potassium atom for a laser linewidth of 10 pm is 1.6×10^{-16} m² so we

Table 1 Returned Signal for Different Operating Conditions

Operating condition	Date	Local time	No. of shots	Observed signal (Rayleigh return) + noise from 31.5 to 35.5 km	Observed signal* (potassium return) + noise from 75.5 to 99.5 km	Background noise* adjusted to 24 km gate†
Laser tuned to 769.9 nm	January 25, 1973	0040-0330	200	160 ‡	378 ± 20	246 ± 19
	January 27, 1973	0230-0520	400	1,836	876 ± 30	674 ± 18
	Total		600	1,996	1,254 ± 36	920 ± 27
	February 1, 1973, to February 2, 1973	2220-0433	600	863	911 ± 30	916 ± 15
Laser detuned	February 1, 1973, to February 2, 1973	2220-0433	600	863	911 ± 30	916 ± 15

* The errors given are ± 1 standard deviation.

† Various gate widths used.

‡ Receiver shutter only partly open.

estimate that the column number density of potassium atoms on January 27, 1973, was $9 \times 10^{11} \text{ m}^{-2}$. This density is consistent with twilight photometric data¹²⁻¹⁴.

Figure 1 shows the resonance scattering profile for the heights 75 to 100 km. There is a broad potassium layer with appreciable density over the whole of the 24 km region studied. The height is consistent with twilight data¹²⁻¹⁴, but the large fraction of potassium that is below 84 km is different from that of sodium in the profiles obtained at 51° N (ref. 15) or at 23° S (ref. 6). This may be a consequence of differences in the chemical and diffusive equilibria of sodium and potassium or it may be that sodium was also at a lower height at Jamaica (18° N) on this occasion compared with that measured previously at other latitudes.

It is hoped to extend these measurements to provide a greater height coverage and finer resolution. Comparison with nearly simultaneous sodium profiles will also be attempted.

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F. FELIX
W. KEENLISIDE
G. KENT

University of the West Indies,
Kingston, Jamaica

M. C. W. SANDFORD

SRC Appleton Laboratory*,
Ditton Park, Slough SL3 9JX

Received September 24, 1973.

*Formerly Radio and Space Research Station.

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Tidal Friction

It seems worth while to compare various estimates of the secular accelerations of the Moon's mean motion ($\dot{n}$) and the Earth's rate of rotation ($\dot{\omega}$), and also of the dissipation (dE/dt) in the M_2 oceanic tide. I have recalculated the values given in chapter 8 of ref. 1, and I indicate the consequences of various other estimates in Table 1.

Van der Waerden's value² from ancient observations (back to the ancient Greeks) was found by classifying the data into four intervals. The number of degrees of freedom is therefore small, and a considerable error was more likely than the apparent uncertainty would suggest. In ref. 1 I made some adjustments, which have been further modified to give the result in Table 1. Newton's and Stephenson's values (refs 3 and 4 respectively) are based on much work, but differ rather

seriously, particularly in the results that they give for the apparent secular acceleration $v (= \dot{n} - \dot{\omega}/27.3)$.

Under 'modern observations' the value of n from van der Waerden is essentially that of Spencer Jones, which eliminates variation of $\dot{\omega}$ in the past 250 yr. I have calculated $\dot{\omega}$ and dE/dt from it on the supposition that the angular momentum in the Earth-Moon system and the Earth's moment of inertia are constant. Spencer Jones's uncertainties may have been underestimated (see ref. 10, page 400). Newton's satellite value⁵ is based on perturbations of artificial satellites by the potential due to the tides; he gives the lunar contribution to $\dot{\omega}/\omega$ and I have calculated $\dot{n}$ and dE/dt above from it. The value of $\dot{\omega}$ calculated from $\dot{n}$ would be increased numerically by allowance for the solar tide and reduced by the thermal atmospheric tide. It seems clear that $\dot{\omega}$ varies, and only an average value means much, but there is no apparent reason why $\dot{n}$ should have varied appreciably in the past 2,000 yr.

Table 1 Consequences of Other Estimates

Author	$-\dot{n}$ ($1'' \text{ cy}^{-2}$)	$-\dot{\omega}$ ($1'' \text{ cy}^{-2}$)	$-dE/dt$ ($10^{19} \text{ erg s}^{-1}$)
Ancient observations			
Van der Waerden ²	17.9 ± 1.1		
Newton ³	42 ± 4	$1,180 \pm 140$	
Stephenson ⁴	34.2 ± 1.9	$1,213 \pm 57$	5.1
Modern observations			
Van der Waerden ²	22.4 ± 1.8	$1,198 \pm 96$	2.76 ± 2.2
Newton (satellites) ⁵	23.5 to 17.5		2.88 to 2.16
Van Flandern ⁶	52 ± 16		
Tidal theory			
Munk and MacDonald ⁷			3.2
Pekeris and Accad ⁸			6.3
Hendershott ⁹			2.7

Van Flandern⁶ studied occultations, using an atomic standard of time, and consequently his result does not depend on the rotation of the Earth. It covers, however, only a short interval of time and will probably be improved. Pekeris and Accad⁸ made a numerical solution for the M_2 tide, using the actual form of the oceans. They did not allow for the elastic yielding of the solid Earth, and their dissipation should probably be halved. Hendershott's solution⁹ allows for it. It seems probable therefore that the rate of dissipation in the lunar semidiurnal tide is about $2.7 \times 10^{19} \text{ erg s}^{-1}$, and this is consistent with Spencer Jones's $\dot{n}$ and Newton's value from artificial satellites. It seems to me that these are the least likely to be affected by disturbances not yet considered. This is substantially greater than my estimate for the dissipation in ref. 1. To find these I started with a wholly provisional estimate (page 308) of the apparent secular acceleration of the Moon, and neglected tidal dissipation along the open coasts, which is probably much larger than was thought (page 313).

HAROLD JEFFREYS

St John's College,
Cambridge

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Anellotubulates are Manufactured 'Microfossils'

In a world where fossil names become ever more prolific, it is unusual to have the opportunity to reverse the trend. Anellotubulates, "an independent morphologic group of organisms", were first described by Wétzel^{1,2} and later placed by him in the family Mikrocallycidae³. There followed a detailed publication by McLachlan⁴, describing world-wide occurrences of anellotubulates and giving a detailed morphological description of these 'fossils'.

Anellotubulates 'ring-tubers' are tubes of constant or varying diameters, often having characteristic annular thickenings and sometimes a thecal form or termination (Fig. 1). They range up to approximately 1 mm in length and 0.5 mm in diameter. The outer surfaces are creamy white and are often rough. The inner surfaces are smoother and sometimes stained brown. They are well figured by McLachlan⁴ who shows most variations. They are amorphous and electron probe microanalysis indicates iron and phosphorus as principal elements in their composition.

Anellotubulates, although not described as such, had been found by several geologists in Britain, including J. E. Pollard and R. E. Elliott in the early 1960s. They considered the possibility that these structures were microfossils but concluded that they were contaminants. The source of contamination was not identified and the work was not published, but a link between the occurrence of these structures and the use of hydrogen peroxide in the preparation of samples for microfauna was established. Anellotubulates often occurred as encrusting forms on fragments of shale disaggregated by hydrogen peroxide. We became interested in this problem in 1972 when anellotubulates were found in microfaunal preparations of Coal Measures mudstones.

Our experience was the same as that of the earlier British workers, in that anellotubulates were found only when hydrogen peroxide was used in the preparation of samples. This fact together with the world-wide distribution of these 'fossils' at stratigraphic horizons varying from Carboniferous to Cretaceous, and their apparent disregard for any particular ecological niche, reinforced in us the suspicions of earlier workers.

Contamination of samples by living organisms either at source or during processing was considered and rejected, for although anellotubulates bear a superficial resemblance to parts of lichens and fungi, no modern species was directly comparable. Experiments introducing modern lichens into the normal preparation of microfaunal samples using hydrogen peroxide failed to mineralise the plant structures. Contamination of hydrogen peroxide by living organisms during its production or afterwards was discounted because of its inhospitable nature.

We suspected that an inorganic source was the most likely origin for these structures, and that they might be produced as artefacts during the reaction between hydrogen peroxide and rocks containing finely disseminated pyrite. A simple experiment proved this to be correct. A piece of Coal Measures shale containing finely divided pyrite was placed in a watchglass and immersed in 100 vols hydrogen peroxide (commercial grade). Observations under a binocular microscope indicated the following sequence of events. Bubbles accumulated on the shale, originating on and below the surface, eventually leading to streams of bubbles as the reaction proceeded. After a period varying from a few minutes to several hours a transparent to white gel-like deposit appeared around the edges of streaming bubbles. Where bubble streaming was persistent, we observed that the deposit formed a tubular growth around and parallel to the bubble stream, the internal diameter of the tubes being directly related to that of the emergent bubbles, and that the original gel-like deposit appeared to harden around the bubble stream, enabling the deposit to build up into a tube-

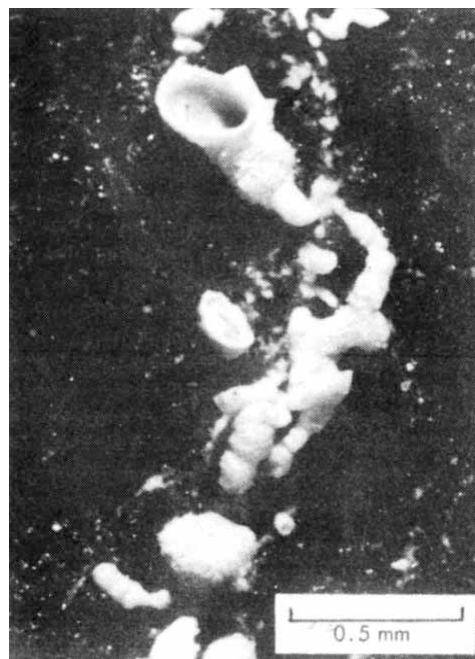


Fig. 1 Anellotubulates grown on carbonaceous Coal Measures shale soaking in 100 vols hydrogen peroxide. Upper centre, large specimen showing early narrow tube swelling into a distorted trumpet form with smooth interior and pronounced rim. After growth a slow stream of expanding bubbles was observed being released from the wide aperture. Left centre, short trumpet form with partially blocked distal end and slit-like aperture. Right centre-lower centre, adherent, constricted and inflated tubes attached along a pyrite-rich layer in shale. These tubes show a white granular external surface. Lower left, narrow tube expanding into a short cylindrical tube.

like growth. As the tube hardened at its base, constricting the diameter of the bubbles, the softer, upper portion of the tube expanded, conforming to the increasing circumference of the bubbles. Trumpet-shaped tubes, formed by this process, were common. Vermiform tubes of an even diameter were also produced. A typical assemblage is seen in Fig. 1. Variations in the quantity of gas produced, and in bubble diameter, resulted in tubes of variable diameter. The hardening of the tubes, while still immersed in the reagent, was progressive and after several hours they were quite brittle.

Production of these artefacts would seem to be dependent on the type of hydrogen peroxide used. Enquiries revealed⁵ that phosphates are often used as stabilising agents in hydrogen peroxide. Commercial and pharmaceutical grades contain as much as 100 or more times the amount present in analytical grades. In our experiments tubes could not be consistently produced using commercial or pharmaceutical grades of hydrogen peroxide, though all tubes that were manufactured were made with these grades. Analytical grade hydrogen peroxide never produced tubes, and this prompted the idea that phosphate in hydrogen peroxide was the source of the phosphorus found in anellotubulates. This idea was reinforced when it was found that if sodium hexametaphosphate was added to hydrogen peroxide abundant tube growth took place. Using sodium hexametaphosphate, tubes were produced on limestone, dolerite (Whin Sill), garnet-pyrite vein rock associated with Shap Granite, and Carboniferous mudstone.

The tubes produced in our experiments embrace all the shapes previously described for anellotubulates, and seem to have the same chemical composition. Those formed without the addition of sodium hexametaphosphate fall within the size range described by Wetzels^{1,2} and McLachlan⁴, but much larger tubes are produced when this chemical is added.

Hydrogen peroxide reacts in the breakdown of samples by oxidising any pyrite present. The chemistry of the reactions, which result in the gel, probably an iron phosphate, being deposited around areas of oxidising pyrite, is still under investigation. There would seem, however, to be little doubt that the elements forming anellotubulates are obtained from the iron of pyrite and the phosphorus of the hydrogen peroxide stabiliser.

The consistent reproducibility of shapes in an amorphous compound during a chemical reaction is an interesting phenomenon. Our research to date would indicate that the iron phosphate is deposited as a gel which seems to have adherent and elastic properties, and which hardens progressively as the reaction proceeds. The streaming bubbles—probably of oxygen—would seem to act in two ways; in distributing the iron from its source at the pyrite to the site of its reaction with the phosphate, and also in physically moulding the gel. That these factors are interrelated is indicated by the final dominance of certain forms as a result of the normal course of this reaction. Assuming that the phosphate is available in decreasing amounts until exhausted during the reaction, the characteristic ring-like thickenings probably relate to periodic fluctuation in the availability of iron from the pyrite source.

We have, therefore, no doubt that anellotubulates can be manufactured as inorganic artefacts, and are led to suspect that all such 'fossils' described in the literature have been so produced. One discrepancy remains to be explained, however. This is a record by McLachlan⁴ of anellotubulates recovered from Australian Triassic sediments using monochloroacetic acid in one case, and boiling water in another, as disaggregating agents. The anellotubulates were apparently recovered by these means by P. J. Jones of the Bureau of Mineral Resources, Canberra, in 1966 and donated to McLachlan in 1971. We have been unable to produce tubes by either of these methods nor can we explain their production during the breakdown of rocks by these means.

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G. RICHARDSON
D. GREGORY

*Institute of Geological Sciences,
Ring Road, Halton,
Leeds LS15 8TQ*

*Department of Geology,
The University,
Manchester 13*

Received September 12, 1973.

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Flocculation of Suspended Sediment in the Sea

INORGANIC sediment suspended in the sea is unstable as single mineral particles and occurs as flocculated aggregates with settling speeds many times greater than that of the constituent grains. This has been demonstrated both through observations of natural samples¹⁻⁵ and through laboratory experiments^{6,7}. In near-shore waters with high concentrations of terrestrially derived mineral matter the transport and deposition of fine sediment are greatly affected by flocculation. The exact

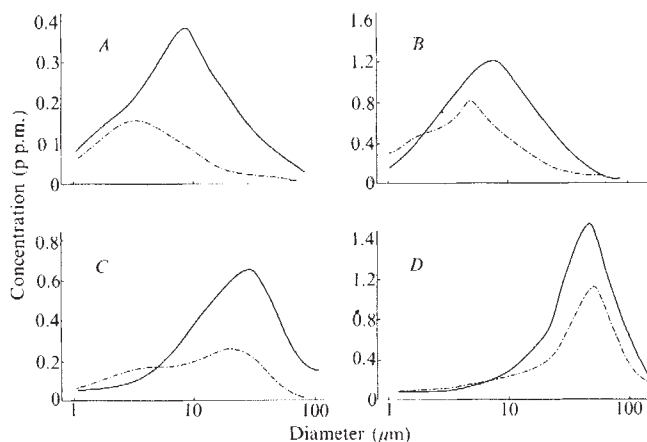


Fig. 1 Typical grain size spectra of particulate matter from coastal waters with high inorganic content. The solid curves show the natural distributions and the broken curves the deflocculated inorganic grains. Origin of samples: A, Northumberland Strait; B, Bay of Fundy; C and D, Petpeswick Inlet.

mechanism of floc formation in the sea is not well understood. The concentration, mineralogy and grain size of the suspended particles and the salinity, turbulence and temperature of the water are commonly regarded as controlling factors^{8,9}. Flocculated aggregates composed largely of organic matter have been observed in the sea^{10,11} and organic matter may be an important ingredient in the flocculation of inorganic sediment.

To determine the nature and mode of formation of inorganic flocs, the concentration, grain size and general nature of the suspended particulate matter in over 500 samples from a variety of coastal environments have been studied. Samples of seawater were collected with Niskin bottles or with a weighted surface sampling bottle. The samples were stored cold and dark and analysed as soon as possible but always within a few days of collection. An inverted phase contrast microscope was used for qualitative identification of the particulate matter. The size and concentration of particles between about 1 and 100 μm were measured with a model T Coulter Counter and the results plotted as particle spectra showing the equivalent spherical diameter of the particle volume relative to concentration in parts per million (p.p.m.)^{12,13}.

Under the microscope flocs appear as irregular masses or clumps of numerous individual grains. Occasionally whole planktonic cells (or parts of a cell) and other organic debris are included but most of the aggregates consist of smooth transparent sub-rounded mineral grains. The flocs form the largest particles in all samples but also range down to minute aggregates of the smallest grains. They are composed of an unsorted mixture of large and small grains. Individual grains not forming part of flocs also occur. These are more abundant among the larger grains and the largest grains in a distribution never seen to become part of a floc.

When gently shaken the bigger flocs move about and consist of a relatively low density, soft, jelly-like, loose network of grains. This conforms with Berthois's description¹ of aggregates of grains surrounded by a fine veil of organic matter.

Flocs were observed in all samples and formed the majority of particles in samples from highly turbulent areas, such as tidal inlets and narrow straits, and from near the bottom in regions of muddy sediment. The samples from these waters, high in inorganic sediment when analysed by Coulter Counter, were characterised by regular, almost symmetrical, broad distributions with well developed modes (Fig. 1). Total particle concentrations in the flocculated samples varied from 0.5 to 20 p.p.m. and modes usually ranged between 6 and 50 μm .

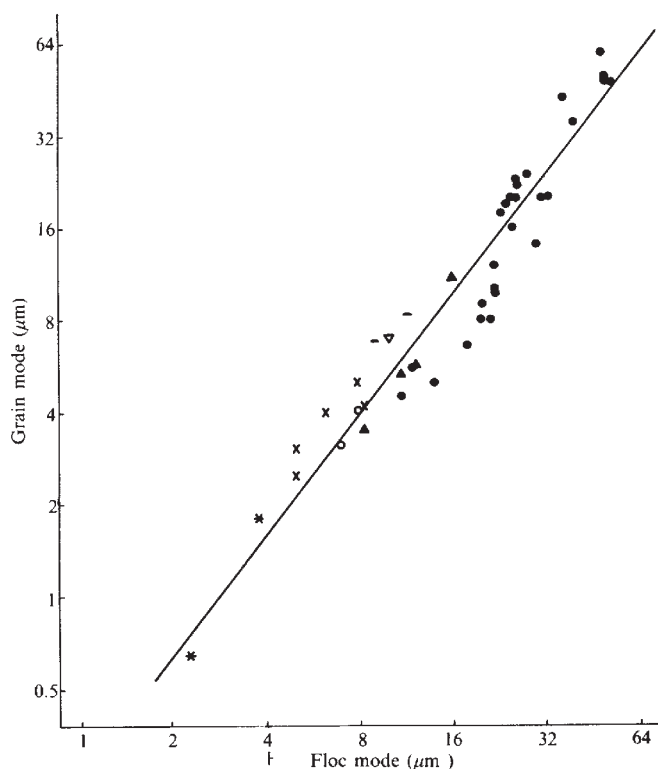


Fig. 2 Relationship between grain size mode and floc size mode ($r=0.941$, $P<0.001$). *, Artificially flocculated samples; ●, Petpeswick Inlet; ▲, Northumberland Strait; —, St Margaret's Bay; ○, Nova Scotia Shelf; ×, Bay of Fundy; ∇, Chedabucto Bay.

These normal distributions seem to be a permanent stable state of thoroughly flocculated samples in which no further flocculation occurred. Vigorous agitation of samples did not change the size distribution; and the suspended floc mode as measured immediately after sampling did not change after storage for several months, during which time the particles settled on the bottom of the container in large lumps or rollers.

The sizes of the inorganic constituent grains forming the flocculated masses were measured as follows. About 500 ml of sample was filtered on to a Millipore filter and ashed in a Tracerlab low temperature ashier. This oxidised the filter paper and organic particles. The residue was wetted with 2% Calgon solution and then resuspended in particle-free 3% saline solution prepared by repeated filtering until the particle concentration was <0.005 p.p.m. The samples were deflocculated by ultrasonic vibration with the probe tip of an Artek Sonic Dismembrator for 1 min at a setting of 90. A microscope was used to check for incomplete dispersion or reflocculation of the inorganic particles before analysis.

When the samples were deflocculated the size spectra changed to a broader flatter distribution, usually slightly negatively skewed. The total particle volume was reduced by about half probably as a result of the loss of porosity or organic material or both. The most remarkable change in the spectra was a change in the mode, relative to the size of the original mode. The grain mode of the samples analysed is shown in Fig. 2 as a function of the original floc mode. The points from the natural samples in Fig. 2 fit the regression equation $\log(\text{grain mode}) = -0.578 + 1.295 \log(\text{floc mode})$.

There was no relationship between the mode size and the concentration of particulate matter, nor did any difference in mineralogical composition between the samples or in the salinity and temperature of the seawater, because of their varied sources, seem to have affected the flocculation process. A distinct relationship was noted, however, between mode size and current speed of the water at the place of collection. Samples with modes below about $14 \mu\text{m}$ came from the turbid

bottom layer of the shelf or from sheltered smaller bodies of water and, in the case of the Bay of Fundy, from close to the shore during slack tide. The samples with large modal size were collected from a narrow inlet with strong tidal currents where, during sampling over a tidal cycle, the mode size varied regularly with changes in the tide. The correlation between floc mode and dynamic conditions is not surprising in view of the dependence of grain size on turbulence¹⁴.

From the above observations it seems that in seawater particles flocculate into characteristic stable size distributions, the modes of which are determined by the initial size of the constituent grains. Flocculation of particles depends on two factors: particle collision during transport and adhesion of particles on contact^{15,16}. Fulfilment of the second condition has been demonstrated by many workers who have shown that in salinities above about 3‰ most fine sediment flocculates readily^{6,7}. I propose the following general mechanism whereby particle collisions transform a suspension of single grains of varying sizes into a permanently flocculated distribution.

Particles of different sizes will respond differently to gravity and turbulent forces. In the initial unflocculated suspension this will cause a random variation in grain speed and direction accompanied by abundant particle collisions. Because of their larger relative surface area and proportionally greater adhesive force the smaller particles flocculate most rapidly¹⁷. Larger grains may not be sufficiently surface active to flocculate with other single grains but they do adhere to the soft irregularly shaped flocs composed of many smaller grains. As progressively larger flocs are formed the transport velocities become more uniform and particle collision less frequent. Eventually a state is reached in which the transport velocities of the largest flocs equal the velocities of the largest grains and further flocculation ceases.

In the sea, particles are suspended in a turbulent medium with local velocity gradients and fluctuations. The forces acting on a particle change constantly and it may be that the stage at which all particles have the same dynamic transport velocities is never achieved. As flocs become larger they are subject to more shear stress and become more unstable, and a bigger increase in grain mode is required to produce a given increase in floc mode. This explains why the ratio of floc mode to grain mode decreases with increase in particle size (Fig. 2).

To test the sequence of flocculation in a sediment suspension a sample of fine glacial clay was dispersed in 3% NaCl solution

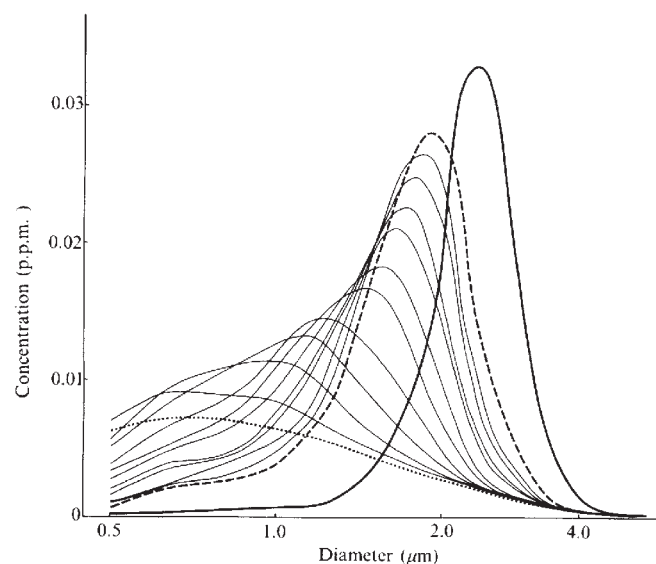


Fig. 3 Progressive flocculation of sediment stirred in 3% NaCl shows the final distribution (18 h) and the distribution measured every 0.5 h during the first 6 h. . . ., 0 h (initial); ---, 6 h; —, 18 h (final).

and stirred continually. Periodic measurement of the grain size shows (Fig. 3) that flocculation proceeds as a front along the distribution, involving progressively larger grains. After 18 h the suspension stabilised in a broad distribution similar to those formed by natural flocs. Further stirring resulted in a decrease of particle concentration probably on account of the adherence of the surface active particles to the walls of the glass container; there was, however, no further change in mode. The final stable mode conforms to the condition of stable flocculation illustrated in Fig. 2. The artificially produced floc distributions appeared identical, optically, to the natural distributions and it may be concluded that the experiment simulated floc formation in nature. The other datum point in Fig. 2 for artificially flocculated samples was similarly derived from a sediment sample stirred in clean seawater. So far no thoroughly flocculated suspensions with modes larger than 4 μm have been produced in the laboratory, possibly because of some deficiency in the organic matter required to duplicate the natural grain surfaces.

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KATE KRANCK

Atlantic Oceanographic Laboratory,
Bedford Institute of Oceanography, Dartmouth, Nova Scotia

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BIOLOGICAL SCIENCES

θ -bearing Lymphocytes in Nude Mice

MICE homozygous for the mutation *nude* (*nu/nu*) are hairless¹ and deficient in thymus²⁻⁴ and thymus-derived (T)^{5,6} lymphocytes. They are generally referred to as being athymic and are widely used in immunology as a source of B lymphocytes uncontaminated with T cells, although there is evidence that the thymus is present, but is small and dysplastic^{3,4}. Indirect evidence has suggested that at least some nude mice have small numbers of T cells⁷⁻⁹, but this has been controversial. I report here the direct demonstration by immunofluorescence of a small population of θ -bearing cells in lymph nodes and spleens of nude mice using fluorescein-conjugated anti- θ antibody.

Anti- $\theta\text{C}_3\text{H}$ antiserum was prepared by immunising AKR mice with 10^7 C_3H MLV-Moloney lymphoma cells weekly for 6 weeks, followed by a further three boosts with 10^7 lymphoma cells and 10^7 C_3H thymocytes. The last inoculation included 4×10^6 Sarcoma 37 cells to induce ascites fluid which was collected 10 d later. Following heat inactivation for 30 min at 56°C , the immunoglobulin was precipitated with 40% saturated NH_4SO_4 and conjugated with fluorescein isothiocyanate¹⁰. After absorption (twice with 50% (v/v) packed AKR thymocytes) to remove autoantibodies, the conjugate used neat or diluted 1:2 (see below), labelled 100% of thymocytes and 35-40% of nucleated spleen cells of Balb/c and A mice, both of which express the $\theta\text{C}_3\text{H}$ allele, but did not label any cells in thymus or spleen of AKR or A/ θ AKR mice (Table 1), which carry the other θ allele, θ AKR. (A/ θ AKR are congenic mice differing from A only at the θ locus.) Absorption with A brain, but not A/ θ AKR brain, completely eliminated the fluorescence labelling activity of the conjugate, further confirming its specificity for $\theta\text{C}_3\text{H}$ (ref. 11).

Six nude mice from three different sources were studied (Table 1) and in each animal no obvious thymus tissue could be seen at the time of killing. Their lymph nodes (inguinal, axillary, brachial, mesenteric, deep and superficial cervical) and spleens were teased in RPMI 1640 medium with 20 mM HEPES buffer and 10% foetal calf serum, passed through a short column of glass wool and washed twice. Spleen cells were freed of erythrocytes by treatment with Tris-buffered NH_4Cl (ref. 12). Twenty microlitres of cells ($4 \times 10^7 \text{ ml}^{-1}$) was incubated with neat conjugate for 30 min at 20°C or 37°C , washed twice and examined under a coverslip with a modified Vickers M41 Photoplan fluorescence microscope, using incident blue-violet light or transmitted phase-contrast illumination¹³.

Between 0.2 and 1.6% of nucleated cells from lymph nodes and spleen of each nude mouse showed surface fluorescent staining (Table 1). Approximately 50% of the labelled cells appeared to be medium or large lymphocytes while the remaining 50% looked like small lymphocytes. As previously described with fluorescein-conjugated anti- θ antibody¹³, labelled cells always showed continuous smooth ring fluorescence, even when labelling was carried out at 37°C .

The specificity studies on the anti- $\theta\text{C}_3\text{H}$ -fluorescein conjugate (Table 1) make it clear that the labelled cells seen in nude lymphoid tissues were θ -bearing. Although epidermal cells¹⁴, fibroblasts (P. Stern, unpublished), and as yet unidentified cells in the central and peripheral nervous system¹¹ are known to express θ on their surface, in cell suspensions of lymphoid tissues, only thymocytes and T lymphocytes have been shown to be θ^+ , while B cells, granulocytes, macrophages, reticular cells and erythrocytes are θ^- (refs 6, 7, 11, 15, 16). This, together with the morphological appearance of the labelled cells by phase-contrast microscopy, makes it likely that the θ^+ cells were T lymphocytes. This is consistent with previous indirect evidence for the presence of small numbers of cells with T lymphocyte properties in nude mice⁷⁻⁹.

What is the origin of the θ^+ lymphocytes in nude mice? Since maternal cells crossing the placenta would have an unusually high probability of surviving in an immunologically incompetent nude mouse^{17,18}, the θ^+ cells could be of maternal origin; although rigorous attempts to demonstrate passage of cells from mother to foetus in mice using chromosome markers have generally failed¹⁹, Di George has referred to a child with thymic dysplasia having 2% maternally derived cells in its blood²⁰. It is unlikely that θ^+ lymphocytes could come from normal (+/+) or heterozygous (*nu*+) littermates as placentae in mice are rarely fused²¹. In view of recent evidence that θ^- cells in mouse bone marrow and spleen can be induced to express θ by *in vitro* incubation with a soluble extract from mouse

Table 1 Percentage of θ -bearing Cells in Control and Nude Mice as Detected with Anti- θ C3H-Fluorescein

Mice	Tissue	Cells scanned*	No. labelled	% labelled
Balb/c	Thymus	200	200	100
	Spleen	200	78	39
A	Thymus	200	200	100
	Spleen	200	72	36
A/ θ AKR †	Thymus	10,000	0	0
	Spleen	13,000	0	0
AKR	Thymus	15,000	0	0
	Spleen	12,000	0	0
OLAC Nude 1 ‡	Lymph node	4,000	60	1.5
	Spleen	3,000	24	0.8
2 ‡	Lymph node	6,900	36	0.5
	Spleen	7,200	37	0.5
3 ‡	Lymph node	1,500	24	1.6
	Spleen	14,000	48	0.3
Nude/C57Bl/6 1 §	Lymph node	19,500	100	0.5
	Spleen	22,000	90	0.4
2 §	Lymph node	20,000	220	1.1
	Spleen	13,000	43	0.3
Nude/Balb/c ¶	Lymph node	5,200	23	0.4
	Spleen	17,000	28	0.2

* Cells were scanned for surface fluorescence with a $\times 100$ oil immersion objective under blue-violet light and the total number of cells scanned was estimated by counting cells under phase contrast in one out of every ten fields examined.

† A/ θ AKR are congenic with A, differing only at the θ locus, and were originally obtained from Dr E. A. Boyse.

‡ Mice were purchased from OLAC (Southern) Ltd (Bicester), where they were produced by mating homozygous males (*nu/nu*) with heterozygous females (*nu/+*) and reared under specific pathogen-free (SPF) conditions. They were all females and 6–8 weeks old when used.

§ Mice were obtained from Dr I. Lefkovits, of the Basel Institute of Immunology. They were both males, 10–12 weeks old, reared under SPF conditions and were produced by mating heterozygous (*nu/+*) males (in their eighth backcross generation on to a C57Bl/6 background) with heterozygous (*nu/+*) outbred NMRI females.

¶ Mouse was obtained from Dr L. La Fleur, of the Basel Institute of Immunology, who purchased it from G. L. Blomholtgård Ltd (Ry, Denmark). This nude was a 6-week-old female from a stock that has been backcrossed for 3–5 generations on to a Balb/c background.

thymus²², it is conceivable that θ^+ lymphocytes can develop outside the thymus, perhaps, in nude mice, under the influence of thymus humoral factors derived from normal littermate⁸, maternal or even dysplastic nude thymus tissue *in utero*. Finally, it is possible that θ^+ lymphocytes develop within the dysplastic thymus; although lymphocytes have not been seen in nude thymus^{3,4}, serial sectioning of foetal or newborn glands will be required to exclude this possibility.

These results suggest that most, if not all, nude mice have significant numbers of T cells. Until it is known whether these cells are immunologically competent, it would be wise to interpret experiments involving these mutants with caution.

I thank Dr G. M. Iverson for supplying the anti- θ C₃H serum, Drs I. Lefkovits and L. La Fleur for providing nude mice and Dr J. J. T. Owen for helpful discussion.

Note added in proof. Since submitting this manuscript, it has been brought to my attention that Vassalli and colleagues²³ found that 1% of cells in nude spleen labelled in an indirect immunofluorescence assay with a heterologous anti-T cell antiserum (anti-MSLA). In addition, they found a small number of cells with an ultrastructural morphology which they considered to be characteristic of T lymphocytes.

MARTIN C. RAFF

MRC Neuroimmunology Project, Zoology Department, University College London, London WC1E 6BT

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T Cells in Recognition of Histocompatibility Antigens

THE capacity of lymphoid cells from normal mice to recognise foreign histocompatibility antigens, as shown in the PAR test, might offer a further assay for the function of T cell receptors, if it could be shown that recognition depends on T cells rather than on B cells. (PAR is a product of antigenic recognition elaborated in one-way mixed lymphoid cell cultures.)

Technical details on test conditions and on PAR determinations have been described^{1,2}. The test provides a quantitative measurement of the recognition of alloantigens, present on F₁ hybrid target cells, by lymphoid cells of parental lineage. Interaction with antigen occurs *in vitro* within a few hours, but demonstration of the recognition product has to be done *in vivo* and seems to need the magnifying effect of a granulotactic mediator, possibly an antibody-antigen complex. The technical disadvantages of the test system are heavily outweighed by its extreme sensitivity³. A major drawback has been, however, that the cell type responding to histocompatibility antigens has been poorly defined. Using cells from various lymphoid organs of normal mice, I here report studies on the critical role of T and B cells in the formation of PAR.

CBA lymphoid cells or spleen cells from Balb/c or from nude (*nu/nu*) mice (backcrossed for seven generations to Balb/c) were used as aggressors (10^6 cells) and confronted *in vitro* with C57Bl/6 antigens present on 10^6 (C57Bl/6 $\times$ CBA)F₁ or (Balb/c $\times$ C57Bl/6)F₁ target spleen cells, respectively. Culture supernatants were collected after various intervals and tested for PAR by injection into the skins of hamsters and determination of the number of accumulated polymorphonuclear cells found 24 h later in trypsinised skin reactions. Only intervals resulting in peak responses are shown. Activation of thymus cells used as thoracic duct lymphocytes (T.TDL) was as described⁴ (see also Table 1). Activated thymus cells (ATC) recovered from spleens (Table 1) and the treatment of cells with anti- θ C₃H and complement (1:10) were also as described⁵ (see also Table 1). Sensitised spleen cells (CBA anti-C57Bl/6) were obtained from CBA mice 21 d after full-thickness C57Bl/6 skin grafts.

Results on PAR assays are shown in Table 1. Balb/c spleen cells reacted to C57Bl/6 antigens maximally within 4 h at 37° C, but spleen cells from nude mice failed to

Table 1 Capacity of Various Lymphoid Cells to Recognise Alien Transplantation Antigens (PAR Assay)

Aggressor cells	Recognition system	Target spleen cells	Peak response (h at 37° C)	PAR ± s.e. * at peak response
Balb/c spleen } nu/nu spleen }		(Balb/c × C57Bl/6)F ₁	4 (None after 2, 4, 7)	10.01 ± 0.08 0
CBA spleen		(C57Bl/6 × CBA)F ₁	7	8.41 ± 0.33
CBA bone marrow			7	2.31 ± 0.12
CBA TDL			7	7.69 ± 0.29
CBA anti-C57Bl/6 spleen			1	16.39 ± 1.25
CBA native thymus			(None after 1, 4, 7, 10)	0
CBA ATC (spleen) †			7	7.83 ± 0.58
CBA ATC (spleen) ‡			7	9.06 ± 0.84
CBA ATC (T.TDL) §			1	12.15 ± 0.46
CBA spleen + anti-θC3H 1:8 to 1:32 + C'			(None after 7)	0
CBA bone marrow + anti-θC3H 1:10 + C'			(None after 1, 2, 4, 7, 10)	0

* Number of PMN cells per skin site. Values corrected for syngeneic controls¹.

† Activated thymus cells. Activation: 10⁷ thymus cells (5- to 6-week-old CBA) injected intraperitoneally into 850 r (C57Bl/6 × CBA)F₁ mice. Spleens collected 5 d later⁵.

‡ Activated thymus cells. Activation according to and by ref. 4, but used as day 5 spleen cells.

§ Activated thymus cells. Activation⁴: 10⁸ thymus cells (5- to 6-week-old CBA) injected intravenously and 10⁸ thymus cells injected intraperitoneally into 750 r (C57Bl/6 × CBA)F₁ mice. Cannulation of thoracic duct on day 4 and for 24 h.

react, presumably because of a lack of T cells⁶⁻⁸. Taking the response of spleen cells in the recognition system CBA to C57Bl/6 as standard, recognition of CBA thoracic duct small lymphocytes (TDL) was very similar and contrasted well with CBA bone marrow cells, known to contain only few T cells^{9,10}, and from native thymus cells which seem to contain either extremely few antigen-sensitive T cells or only precursors of such cells. In sharp contrast to the response of native thymus cells, ATC reacted well. Differences were observed, however, depending on whether ATC were collected from thoracic duct or from spleen. The former (T.TDL) displayed a response characteristic of sensitised cells (CBA anti-C57Bl/6 spleen cells) with peak formation of PAR and with large amounts after cultivation for only 1 h (ref. 11). ATC collected from spleen behaved like normal spleen cells and both activation procedures^{4,5} gave identical results. The different behaviour of ATC seems to indicate that the cells follow distinct pathways in irradiated F₁ hosts. Cells differentiated to normal competence (or possibly returned to it after passing through an effector stage) settle in the spleen; cells differentiated to killer lymphocytes circulate and can be collected from the thoracic duct. Corroborating these results are those on cell suspensions in which T cells have been eliminated by treatment with anti-θ C3H serum and complement. B cells from spleen or from bone marrow were incapable of PAR formation.

Together with previous work^{1,2,5}, my results indicate that recognition of histocompatibility antigens by lymphoid cells as measured in the PAR assay is a T cell function. Antigenic response was lacking or reduced if aggressor cells contained no, or only few, T cells. The PAR assay failed to measure recognition of alloantigens by B cells. In line with other T cell functions, recognition of transplantation antigens is an extremely specific process^{1,3} and seems to be a prerequisite for manifestations of immunity¹² and tolerance¹³.

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H. RAMSEIER*

Basel Institute for Immunology, Basel,
and Department of Medical Microbiology,
University of Zürich

Received July 19; revised August 23, 1973.

* Present address, and address for reprint requests: Division of Experimental Microbiology, Department of Medical Microbiology, University of Zürich, Gloriastrasse 32, CH-8006 Zürich.

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Differences between Cytoplasmic Glucocorticoid-binding Proteins shown by Heterogeneity of Nuclear Acceptor Sites

It is now widely accepted that most, if not all, glucocorticoid responses in target tissue are mediated by specific steroid receptor molecules in the cytoplasm^{1,2}. Following association between steroid and receptor, there is transport of the steroid to the nucleus where subsequent interaction with the genome presumably alters transcriptional processes³. This scheme does not yet satisfactorily explain the widely differing responses of various tissues to glucocorticoids. Specificity of the kind of response observed could lie in differences in the receptor molecule itself and/or in the ability of a common receptor molecule to interact only with certain 'exposed' parts of the nuclear material in a given cell. Some evidence has favoured the latter interpretation⁴, but there has not yet been support for the former contention. In this report, we present evidence that at least some degree of specificity of response may be mediated by subtle differences in the cytoplasmic corticosteroid-binding proteins between tissues with differing kinds of responses to glucocorticoids. Our data further suggest that there are intranuclear binding sites for cytoplasmic glucocorticoid receptors which are of high affinity and saturability and which can distinguish between glucocorticoid receptors of differing origin.

In these studies, hepatoma tissue culture (HTC) cells and mouse fibroblasts (L cells) were grown in tissue culture and collected by standard methods^{5,6}. Both HTC⁷ and L cells⁸

have high affinity cytoplasmic receptor molecules and there is considerable evidence to implicate these proteins in the glucocorticoid response of both tissues. The responses themselves are dissimilar with specific increases in enzyme synthesis⁹ and in Phe-tRNA¹⁰ noted in HTC cells⁹ and a general inhibition of macromolecular synthesis in L cells¹¹. Neither of these cells' steroid binding molecules has been extensively characterised. As Fig. 1 shows, the binding affinities of the binding protein from HTC and L cells for the glucocorticoid dexamethasone are very similar. There does not seem to be significant interaction between cytoplasmic receptors from HTC and L cells because mixes of the two cytosols show essentially additive binding of glucocorticoid without obvious alteration in affinity.

Similarly, efforts to distinguish between HTC and L cell receptors by differences in heat inactivation showed small differences at 30° C (Fig. 2), and no differences at 34° C. Physicochemical characterisations of other glucocorticoid receptors have revealed, in general, close similarity¹².

To examine better the physiological properties of the cytoplasmic receptors from these two cell types we studied their uptake and binding to isolated nuclei in an *in vitro* system. Nuclear binding is a temperature-dependent process presumably involving a transformation in the cytoplasmic receptor. At 20° C we observed a depletion of cytoplasmic receptor in this *in vitro* system far greater than that due to heat inactivation and occurring coincidentally with the appearance of counts in the nuclei. Others have not found it necessary to warm the nuclei to obtain uptake of the temperature-transformed cytoplasmic receptors¹. Results at 0° C and 37° C are similar to those reported before¹³.

Nuclear binding of cytoplasmic receptor-steroid complex is of high affinity, and in our *in vitro* system shows saturability (Fig. 3). If it be assumed that one steroid molecule is bound per molecule of cytoplasmic receptor then the molar concentration of receptor in the incubation mix can be calculated. It can be seen that increases in homologous cytoplasmic receptor concentration above about 2.0 nM do not result in further increases in nuclear uptake of steroid. There is virtually no uptake of steroid by the nuclei in the absence of added cytoplasmic receptor. The values obtained for number of sites and affinity for HTC cells are similar to those reported before¹³. Both HTC and L cell nuclei have similar numbers of binding sites for their homologous receptor proteins on a per mg of protein basis. These sites may involve DNA, chromatin proteins or both. Work by others suggests that the nuclear sites for HTC cells may

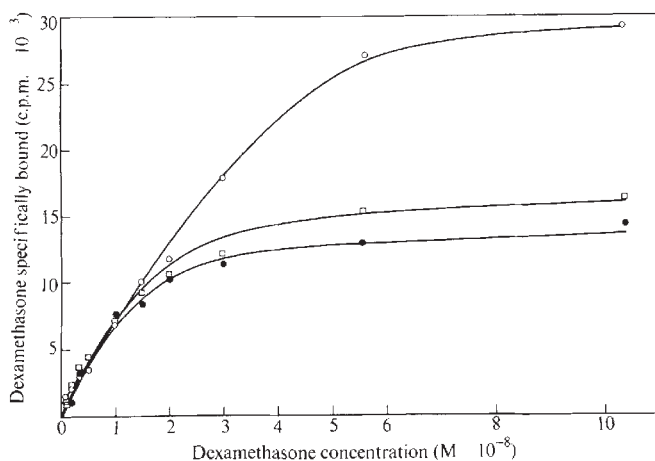


Fig. 1 Binding of ³H-dexamethasone (Amersham, 24 Ci mmol⁻¹ used throughout) to cytoplasmic receptors from HTC (●) and L cells (□); and to a mixture of HTC and L cell cytosols (○). Preparation of cell extracts and binding reactions were carried out using methods described previously¹². HTC⁹ and L cells⁶ were grown in suspension culture using published techniques.

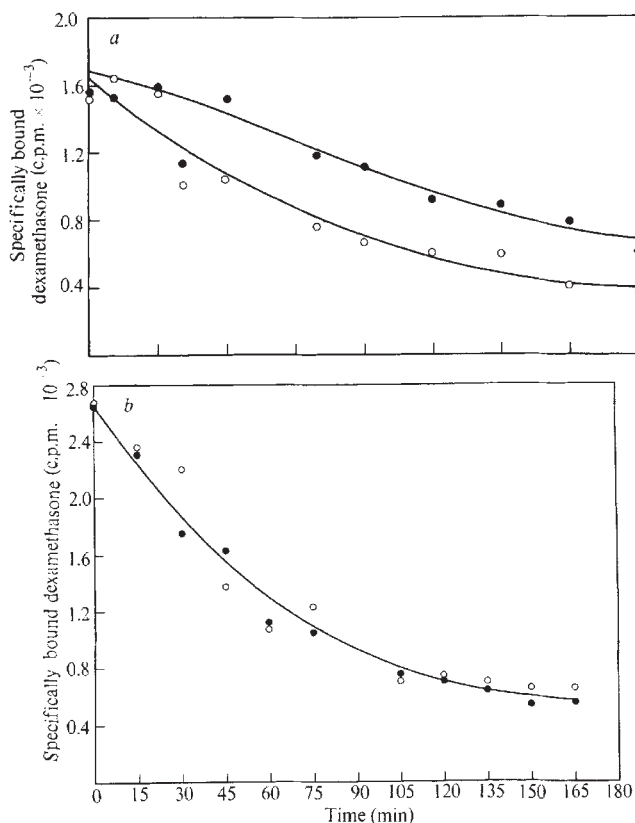


Fig. 2 *a*, Heat inactivation of HTC cell (●) and L cell (○) cytoplasmic receptors at 30° C. Paired samples of cytosol were equilibrated with 8×10^{-8} M ³H-dexamethasone in the presence or absence of 10^{-4} M unlabelled dexamethasone for 2.5 h at 0–2° C. At the start of the experiment the samples were placed in a 30° C water bath and aliquots removed in triplicate from both tubes at various times. Binding studies were performed using a modification of the DEAE-cellulose filter paper assay¹⁴ in which filters were initially washed with buffer I (20 mM Tris-HCl, pH 7.6, 1 mM MgCl₂; 2 mM CaCl₂; and 0.25 M sucrose) and the sample for assay run through the filter slowly after initial dilution in 15 volumes of buffer I. The filters were washed exhaustively with buffer I, air dried and counted in NCS (Amersham)-Liquifluor-toluene in a liquid scintillation counter (efficiency for ³H ~ 20%). Results were comparable to the charcoal assay techniques¹². Counts bound represent triplicate determinations of the difference between total counts bound and the non-competable background (usually less than 20% of the total counts on the filter). *b*, Heat inactivation of HTC (●) and L (○) cell cytoplasmic receptors at 34° C; methods were as described above.

consist of DNA¹³ although progesterone binding in other cell systems may require non-histone chromatin proteins and DNA⁴. Binding of label to nuclei in our *in vitro* system is felt to be specific in that there is an absolute requirement for the temperature-transformed cytoplasmic receptor and the system shows saturability at concentrations of cytoplasmic receptor which seem to be physiologically reasonable.

Since the phenotypic response of HTC and L cells to glucocorticoid is so different, and since biochemically the cytoplasmic receptors appear so similar, we were interested in knowing whether cytoplasmic steroid-receptor complex from one cell could interact with the nuclear sites of the other cell. If so, would there be competition between receptors from different sources for these nuclear sites? That is, once nuclear sites were saturated with homologous receptor protein were there additional sites which could be detected by cytoplasmic receptor from another source? Figure 4 shows that HTC cytoplasmic receptor complexes can bind to L cell nuclei to an approximately similar extent as homologous cytoplasmic receptor (*A*). Similarly, L cell cytoplasmic receptors can bind to HTC cell nuclei to an extent comparable

to that of HTC cell receptors (*B*). Furthermore, these heterologous systems show similar binding affinities and saturation phenomena characteristic of the homologous *in vitro* systems shown in Fig. 3.

Figure 3 shows that homologous nuclear saturation is maintained at a plateau level over a range of steroid-receptor complex concentrations. By saturating nuclei, it is possible to do mixing experiments in which additional steroid-cytoplasmic receptor from another cell is added to the incubation mix. In each experiment a saturation curve with the appropriate receptor complex was done as in Fig. 3 to show that the amount of the initial cytoplasmic receptor was saturating. Under these conditions, additional nuclear binding above plateau levels for the saturating receptor would be detected if the added heterologous receptor were occupying different sites. Interaction between cytoplasmic receptors was thought to be unlikely (see Fig. 1). As Fig. 4 shows, we found that when HTC cell nuclei were saturated with HTC cell receptor, they would bind glucocorticoid receptors from L cells. Conversely, when L cell nuclei were saturated with L cell cytoplasmic steroid binding complexes it was possible to detect additional sites which bound HTC cell receptors. Under our conditions, it was not possible to state whether or not there was any overlap between sites occupied by the two cytoplasmic receptors. Experiments involving sequential nuclear binding with saturating amounts of different receptor are currently under way.

These results suggest that HTC and L cells possess different cytoplasmic receptors which can be distinguished by their

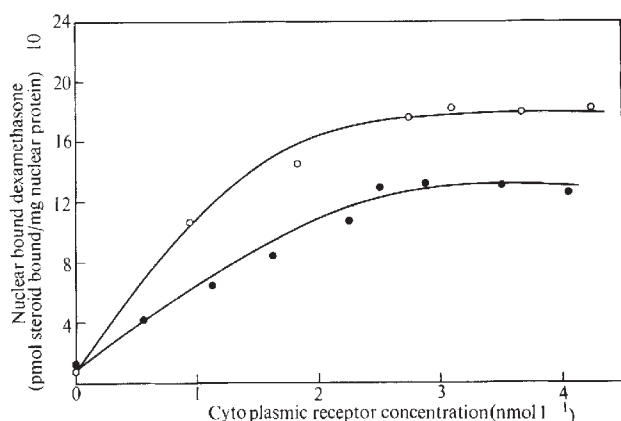


Fig. 3 Nuclear binding of HTC cell steroid receptor complexes by HTC cell nuclei and L cell steroid receptor by L cell nuclei. Cytosol was prepared¹² in buffer I and incubated for 2 h at 0–2° C with 8×10^{-8} M ^3H -dexamethasone in the presence or absence of 10^{-5} M unlabelled dexamethasone. Estimates of cytoplasmic receptor concentrations were obtained using the method described in Fig. 2. Nuclei were prepared by homogenising the cells in a tight fitting Dounce homogeniser in buffer I without sucrose and collecting the nuclear pellet by centrifugation at 800g for 5 min. This pellet was suspended and washed three times in buffer I and then incubated at 0–2° C for 2 h with 8×10^{-8} M ^3H -dexamethasone in the presence or absence of 10^{-5} M unlabelled dexamethasone to compete with the labelled steroid. After preliminary incubation of both nuclei and cytosol with steroid, they were mixed in a ratio of 1:4 (v/v), uncompeted cytosol with uncompeted nuclei and competed cytosol with competed nuclei. Before mixing, cytosol receptor was diluted with varying amounts of buffer I which had been pre-equilibrated with equivalent concentrations of radioactive or radioactive plus unlabelled steroid. After 75 min at 20° C, the nuclei were collected by centrifugation and washed three times in buffer I. The nuclei were sonicated in a small amount of water in a Bronwill sonicator (10 s, setting 15) and a Lowry protein determination¹⁵ was done on an aliquot. A further aliquot was counted in a liquid scintillation counter in 8 volumes NCS to which was added 10 ml of Liquifluor-toluene. In all cases, specifically bound nuclear steroid represents the difference between the total nucleus-bound counts and the counts bound in the competed sample containing unlabelled steroid as well as ^3H -dexamethasone. HTC cell cytosol plus HTC cell nuclei (○); L cell cytosol plus L cell nuclei (●).

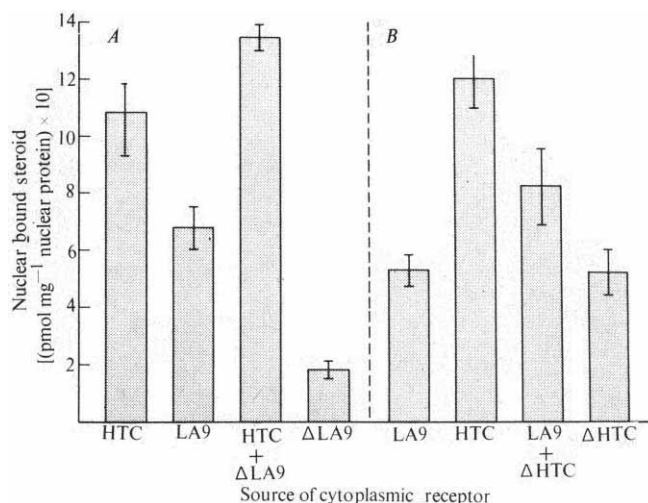


Fig. 4 *A*, Binding of various cytoplasmic receptors to HTC cell nuclei. These experiments were performed in a manner similar to those in Fig. 3. In each experiment a nuclear binding curve as shown in Fig. 3 was generated so that it would be clear that sufficient homologous receptor was present to saturate all nuclear sites to which it would bind. Then an additional amount of L cell cytoplasmic receptor was added to the incubation mix. After 75 min of incubation, nuclear binding was assayed as described in Fig. 2. ΔLA9 represents the increment in nuclear bound counts observed with the small amount of added L cell cytoplasmic receptor. *B*, Binding of various cytoplasmic receptors to L cell nuclei. Techniques similar to those described above were used except that L nuclei were mixed with saturating concentrations of HTC or L cell cytoplasmic receptor and an additional amount of HTC cell receptor was added to the incubation mix containing saturating amounts of L cell receptor. ΔHTC represents the increment in nuclear bound counts observed with the small amount of added HTC cell cytoplasmic receptor.

additive binding to nuclei. An implication is that nuclear binding can be used to assay for qualitative differences in corticosteroid receptors. It also seems that there must be different classes of sites in a given nucleus, able to recognise specific glucocorticoid receptors. It should be realised that the relationship of these nuclear sites, measured *in vitro*, to physiologically significant control sites is unknown. We cannot exclude the possibility that somehow receptor of one cell type facilitates entry of the other receptor, though this seems unlikely.

Previously, the presence or absence of steroid responsiveness has been attributed to the presence or absence of functional cytoplasmic receptor. Such data have not explained the qualitatively different tissue responses to the same hormone. Although the data here were obtained from mouse L cells and rat HTC cells, they nevertheless suggest that qualitative differences in cytosol receptors for glucocorticoids contribute to the specificity of phenotypic response in different tissues.

MARC E. LIPPMAN
E. BRAD THOMPSON

Laboratory of Biochemistry,
National Institutes of Health,
Bethesda, Maryland 20014

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Anticoagulant Action of Heparin

ANTITHROMBIN III (heparin cofactor) is known to inhibit thrombin¹ and Factor X_a¹⁻³ (activated Factor X). We have purified antithrombin⁴ from human plasma by a series of chromatographic and electrophoretic separation techniques. The homogeneity of the final product is demonstrated by disc gel electrophoresis, SDS gel electrophoresis and immunoelectrophoresis⁴. Using this preparation, we have shown that antithrombin forms a 1:1 stoichiometric complex with thrombin which cannot be dissociated with denaturing and reducing agents. Addition of heparin dramatically accelerates the rate of formation of this complex. Complex formation is completely dependent on an interaction between the serine active centre of thrombin and an arginine reactive site of antithrombin. Furthermore, ε-amino lysyl groups of antithrombin serve as binding sites for the highly negatively charged heparin. Based on these data and other evidence, we have proposed that heparin acts to accelerate inhibitor function by binding to antithrombin and inducing an allosteric modification in it, which renders the arginine in its reactive site more accessible to the serine in the active centre of thrombin⁴.

This mechanism suggested that all serine proteases within the coagulation cascade may be neutralised in a similar manner. To test this hypothesis we have examined the interaction of antithrombin and heparin with Factor XI_a (activated Factor XI), another serine protease⁵ generated during coagulation, whose physical properties and substrate specificity differ considerably from those of Factor X_a and thrombin.

Factor XI_a was purified from human plasma by DEAE-cellulose chromatography, celite adsorption-elution, hydroxyapatite chromatography, and polyacrylamide P-150 gel filtration according to the method of Amir *et al.*⁶ with minor modifications. The final product is free of coagulation Factors I, II, V, VII, VIII, IX and X as judged by fibrinogen determinations⁷ and one-stage assays using congenitally deficient plasmas as substrates⁸.

To ascertain whether antithrombin inactivates Factor XI_a, antithrombin was incubated with Factor XI_a at final concentrations of 64 units ml⁻¹ and 100 units ml⁻¹ respectively, and residual Factor XI_a activity was sequentially measured as a function of time. As shown in Fig. 1, there is a progressive decline in Factor XI_a activity. No significant reduction in this activity is, however, observed when buffer is substituted for antithrombin. When the neutralisation of Factor XI_a activity is plotted in semi-log fashion (not shown), the kinetics closely approximate those of a first order reaction.

To determine whether antithrombin is responsible for a significant proportion of the plasma's ability to inhibit Factor XI_a, we removed antithrombin from plasma by specific immunoprecipitation and measured the residual capacity of these samples to neutralise this activated intermediate. Plasmas deficient in C1 esterase inhibitor (C1INH) were employed in these experiments, since it is known to be responsible for 20% of the plasma's Factor XI_a inhibitory capacity¹⁹ and activation of the complement system during immunoprecipitation would result in its reduction. As shown in Table 1, specific immuno-

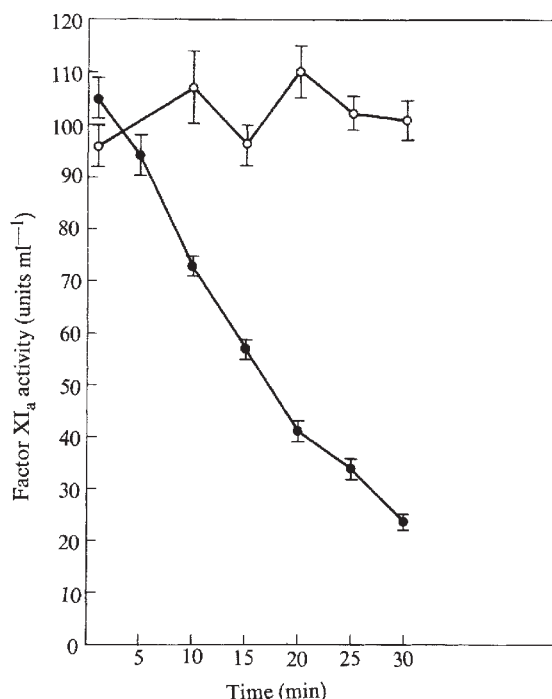


Fig. 1 Inhibition of Factor XI_a by antithrombin. Factor XI_a was incubated with buffer or antithrombin in albuminised plastic tubes at 37° C. 0.02 ml of Factor XI_a was added to either 0.38 ml of 0.15 M NaCl in 0.01 M Tris (pH 7.5) or 0.18 ml of this buffer and 0.2 ml antithrombin. The final concentrations of Factor XI_a and antithrombin used were 100 units ml⁻¹ (0.002 AU ml⁻¹) and 64 units ml⁻¹ (0.060 AU ml⁻¹), respectively. Mixtures were separately incubated for varying periods of time and assayed in triplicate for residual Factor XI_a. Each point represents the mean obtained for two separate incubation mixtures ± s.e.m. ○—○, Factor XI_a+buffer; ●—●, factor XI_a+antithrombin. Factor XI_a activity was measured by a modification of the method of Nossel²¹ utilising cephalin²² and plasmas congenitally deficient in this coagulation protein. The initial concentrations of Factor XI_a preparations were estimated by comparison with the Factor XI content of pooled plasma obtained from twenty-five normal subjects. Assays of Factor XI levels in plasma were performed by the method of Rapaport *et al.*²³ with the mean value of this plasma component arbitrarily set at 100 units ml⁻¹. Antithrombin (heparin cofactor) activity was determined by a modification of the method of Gerandas²⁴ in the absence of heparin or by the method of Abildgaard⁹ in the presence of heparin. Purified human thrombin²⁵ (3,200 NIH units mg⁻¹) was employed in both types of activity measurements. The concentration of antithrombin was also obtained by immunological assay using the technique of Laurell²⁶ with monospecific antisera prepared by immunizing rabbits with the purified protein. Activity and immunological measurements were in excellent agreement and were compared to a pool of twenty-five normal plasmas. Unless otherwise indicated, the concentrations of antithrombin cited represent an average of all types of determinations. The mean concentration of antithrombin was arbitrarily set at 100 units ml⁻¹.

precipitation of 71–80% of plasma antithrombin by a γ-globulin concentrate directed against this protein produces, in all cases, corresponding declines in the ability of these samples to inhibit Factor XI_a. In each instance, non-specific effects of γ-globulin addition have been accounted for by comparing these results with duplicate samples to which we have added a “bland” γ-globulin concentrate obtained from non-immunised rabbits. From these data we estimate that antithrombin is responsible for about 50% of these plasmas’ capacity to neutralise Factor XI_a. Using the data of Ratnoff *et al.*¹⁰, on the relative importance of plasma C1INH as an antagonist of Factor XI_a, we suggest that antithrombin is responsible for about 40% of the Factor XI_a inhibitory capacity of normal plasma, with the residual 40% inhibitory capacity due perhaps to the inhibitor recently isolated by Amir *et al.*¹¹.

These three plasma protease inhibitors only slowly neutralise Factor XI_a activity. If the antithrombin-Factor XI_a interaction

were to be dramatically accelerated by heparin, then the addition of this anticoagulant to plasma would transform antithrombin from a plasma inhibitor of some importance to the primary antagonist of Factor XI_a action. To test this possibility we examined the time-dependent decay of Factor XI_a in the presence of antithrombin and heparin.

Methodological problems make such measurements difficult. Heparin is known to inhibit (through antithrombin) the action of Factor X_a on prothrombin¹⁻² as well as that of thrombin on fibrinogen¹, and a valid determination of the proteolytic activity of Factor XI_a depends on the degree to which this species triggers these terminal reactions of the coagulation cascade. Therefore heparin as well as antithrombin-heparin complexes must be removed from incubation mixtures before assaying them for Factor XI_a. Well-recognised approaches to inhibiting heparin's action such as protamine and heparinase were initially examined. Protamine was found not only to inhibit heparin but also to reduce directly the activity of Factor XI_a. Heparinase at concentrations of 0.13 mg of crude enzyme ml⁻¹ (ref. 12) was shown to rapidly destroy free heparin but only slowly affect heparin bound to antithrombin. The addition of 9 mg of DEAE-cellulose to test incubations, centrifugation of these mixtures and use of the supernatants, proved, however, to be a rapid means of selectively eliminating free or bound heparin without altering the level of Factor XI_a.

Several experiments demonstrated the validity of this approach: First, when heparin and Factor XI_a were incubated together and processed in the above fashion, no significant decrease in Factor XI_a activity was detected (Fig. 2). Second, when antithrombin and heparin were incubated together for 5 min, the mixtures treated with DEAE-cellulose, and the resulting supernatants incubated with Factor XI_a, only a minimal reduction in Factor XI_a activity was seen (Fig. 2). Third, when Factor XI_a was incubated with buffer and treated with DEAE-cellulose, the supernatants obtained showed no significant reduction in the initial Factor XI_a activity (Fig. 2).

To test whether heparin accelerates the interaction between Factor XI_a and antithrombin, mixtures of antithrombin, heparin, and Factor XI_a at final concentrations of 36 units

ml⁻¹, 0.7 units ml⁻¹, and 100 units ml⁻¹, respectively, were incubated together for 1, 3, or 5 min before the addition of DEAE-cellulose. After centrifugation, the respective supernatants were analysed and Factor XI_a activity was found to be virtually absent in all incubation mixtures (Fig. 2). In other experiments (not shown), shorter periods of incubation (0.1 to 0.5 min) revealed the same degree of Factor XI_a inhibition. These results are to be compared with those obtained when the inactivation was examined in identical conditions, but with the omission of heparin. In these mixtures only a minimal time-dependent reduction in Factor XI_a activity was observed (Fig. 2). Therefore heparin dramatically accelerates the formation of a Factor XI_a-antithrombin complex. On the basis of the neutralisation of Factor XI_a by antithrombin in the absence of heparin, as well as our knowledge of the mechanism of inhibitor action, we may confidently presume that inactivation of Factor XI_a follows this interaction.

Thus Factor XI_a, like Factor X_a and thrombin, is a serine protease which is slowly but progressively inhibited by antithrombin in the absence of heparin and virtually instantaneously neutralised by antithrombin in the presence of this anticoagulant. On the basis of these data and the known mechanism of action of antithrombin⁴, we predict that antithrombin will be shown to inactivate the other remaining serine proteases of the coagulation cascade, Factor VII_a (ref. 13), Factor IX_a (ref. 14) and Factor XII_a (ref. 15), and that heparin will accelerate each of these interactions. Indeed, Aronson, using antithrombin prepared by us, has already confirmed this hypothesis for Factor IX_a (personal communication).

Furthermore this linkage of the anticoagulant action of heparin to the early "contact phase" of the coagulation cascade suggests that antithrombin may play a significant role in maintaining nonthrombogenic properties of artificial as well as natural surfaces. When heparin is covalently bonded to a variety of materials, they prove to be highly thromboresistant^{16,17}. It is probable that they owe this property to "activated" antithrombin, bound at the blood-surface interface, which can rapidly neutralise serine proteases generated during the initial contact phase of clotting and thus abort

Table 1 The Factor XI_a Inhibitory Activity in C₁INH-deficient Plasmas Depleted of Antithrombin

Sample	Initial antithrombin concentration (units ml ⁻¹)	Initial C ₁ INH concentration (units ml ⁻¹)	% Antithrombin-heparin cofactor activity removed by immunoprecipitation	Factor XI _a removed after 15 min of incubation at 37° C (units ml ⁻¹)
Buffer	—	—	—	7
C ₁ INH-deficient plasma 1	100	8.7	—	—
a + γ-globulin concentrate directed against antithrombin			80	56
b + "bland" γ-globulin concentrate			0	75
C ₁ INH-deficient plasma 2	100	12.0	—	—
a + γ-globulin concentrate directed against antithrombin			71	39
b + "bland" γ-globulin concentrate			0	73
C ₁ INH-deficient plasma 3	110	19.0	—	—
a + γ-globulin concentrate directed against antithrombin			76	55
b + "bland" γ-globulin concentrate			0	70

Plasmas deficient in C₁INH were obtained from three patients with hereditary angioneurotic oedema using two-syringe technique, plastic vessels and sodium citrate as anticoagulant (0.38% final concentration). They were individually assayed for antithrombin (see legend to Fig. 1) and C₁INH by the technique of Laurell with monospecific sera (normal range: 44–177 units ml⁻¹). Samples were then stored at –70° C. Sera were obtained from rabbits immunised with purified antithrombin as well as rabbits which had not been immunised. The γ-globulin fractions were prepared by the method of Stanworth²⁷, concentrated to 100 AU ml⁻¹, dialysed against 0.15 M NaCl in 0.01 M Tris (pH 7.5) and heated to 60° C for 1 h. These γ-globulin concentrates did not inhibit Factor XI_a nor did they exhibit antithrombin or heparin cofactor activity. Monospecificity of the γ-globulin concentrate directed against antithrombin was demonstrated by immunoelectrophoresis and Ouchterlony double diffusion. C₁INH deficient plasmas (0.3 ml) were added to both types of γ-globulin concentrates (0.9 ml) and the mixtures were incubated for 30 min at 37° C and 90 min at 4° C. The samples were centrifuged at 15,000g and 4° C for 20 min. The resulting supernatants were assayed for residual heparin cofactor activity (measurement of antithrombin activity in the presence of heparin, see legend to Fig. 1). In addition, residual Factor XI_a inhibitory activity was determined by adding 0.2 ml of supernatant to 0.18 ml of 0.15 M NaCl in 0.01 M Tris (pH 7.5) and 0.02 ml of Factor XI_a (final concentration of 100 units ml⁻¹). After 15 min of incubation at 37° C, the mixtures were assayed in triplicate for Factor XI_a activity (see legend to Fig. 1). Controls were obtained for the same period of incubation by replacing the supernatants with 0.15 M NaCl in 0.01 M Tris (pH 7.5). Figures given represent the means obtained for duplicate incubation mixtures prepared on two separate days. The relative Factor XI_a inhibitory capacity of plasmas (not given) was estimated by a plot of plasma dilution against the log of the residual Factor XI_a activity at 15 min.

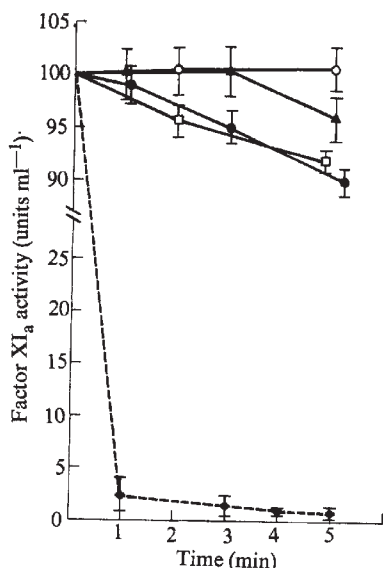


Fig. 2 Inhibition of Factor XI_a by antithrombin and heparin. Two distinct sequences of addition of Factor XI_a to other components were employed to obtain the above data. The first incubation sequence used duplicate samples of 1.6 ml composed of Factor XI_a added to either buffer, heparin, antithrombin or antithrombin and heparin. In this instance, Factor XI_a was employed at a final concentration of 100 units ml⁻¹ while heparin and antithrombin, if present, were used at final concentrations of 0.7 units ml⁻¹ and 36 units ml⁻¹, respectively. All samples were incubated for 1–5 min at 37° C, dried DEAE-cellulose (9 mg) was added with gentle agitation, the mixtures were immediately centrifuged at 15,000g for 30 s and the resulting supernatants were assayed in triplicate for residual Factor XI_a activity. The second incubation sequence used duplicate samples of 1.2 ml consisting of antithrombin and heparin at final concentrations of 48 units ml⁻¹ and 0.93 units ml⁻¹, respectively. Both samples were incubated at 37° C for 5 min, dried DEAE-cellulose (9 mg) was added with gentle agitation, the mixtures were centrifuged at 15,000g for 30 s and the resulting supernatants were obtained. Factor XI_a was added to these supernatants in a 1 : 3 (v/v) ratio such that its final concentration was 100 units ml⁻¹; the samples were incubated at 37° C for 2–5 min, and the residual Factor XI_a activities were determined in triplicate. Values given represent the mean Factor XI_a activity $\pm$ s.e.m. In all cases, components have been either dialysed against or extensively diluted in buffer (0.15 M NaCl in 0.01 M Tris, pH 7.5). ○—○, Factor XI_a + buffer; ▲—▲, factor XI_a + heparin; □—□, factor XI_a + heparin-antithrombin-treated supernatant; ●—●, factor XI_a + antithrombin; ◆—◆, factor XI_a + heparin + antithrombin.

the production of the terminal serine proteases responsible for thrombin formation. By analogy, since heparin has been isolated from vessel walls^{18,19}, as well as other organs²⁰, it may prove fruitful to reinvestigate vascular endothelial thromboresistance in terms of heparin or heparin-like material bound to surfaces. The presence of such substances coupled with the unique properties of antithrombin suggests that this inhibitor is the critical barrier against activated coagulation factors at the vessel wall interface and thus provides a crucial link between surface properties and the coagulability of blood.

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PAUL S. DAMUS
MICHAEL HICKS
ROBERT D. ROSENBERG

Departments of Medicine of the Beth Israel Hospital
and Harvard Medical School,
Boston, Massachusetts

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Perception of Polarised Light in Insects by Filter Mechanism

THE response of a single visual cell of an insect, intracellularly recorded^{1–3}, to polarised light (PL) is relatively small but, as is generally known, insects display a great capability of orientating to polarised light. Here I suggest that this contradiction can be explained with the aid of a filter mechanism.

My theory is based on some well known facts. Many insects have so-called tiered retinulas with one or more basal (or proximal) visual cells and both theoretical⁴ and experimental^{5,6} results indicate that the microvilli of rhabdomeres exhibit dichroism with a dichroic ratio of 2. Furthermore, the longer the dichroic rhabdomere, the lesser its dichroic ratio, which is 1 in the limit because of 100% absorption for any orientation of the E vector. Thus, a long rhabdomere could hardly act as a PL detector in the eye of an insect.

The principal idea presented here is that a distal rhabdomere serves as a PL filter (analyser) for linearly polarised light from blue sky, whereas the proximal one absorbs or measures the intensity of PL transmitted by the distal rhabdomere (Fig. 1).

Let the intensity of incident PL be I_0 . When the E vector of PL is parallel to the long axes of the microvilli, that is, when it is orientated in the x direction, the intensity of light transmitted by the distal rhabdomere and entering the proximal rhabdomere will be $I_{x_d} = I_0 \exp(-x_d) = I_0 \exp(-k l_d)$ where k is the absorption coefficient and l_d is the length of the distal rhabdomere. In turn, $k = 2.3 \epsilon c_x$ where ϵ is the molar extinction coefficient, c_x is the concentration of photopigment molecules orientated in the x direction, and 2.3 is the constant derived from changing from the generally used extinction coefficient $K = \epsilon c_x$ to the absorption coefficient (k) used here; that is

$$I_{x_d} = I_0 \exp(-2.3 \epsilon c_x l_d) = I_0 \times 10^{-\epsilon c_x l_d}$$

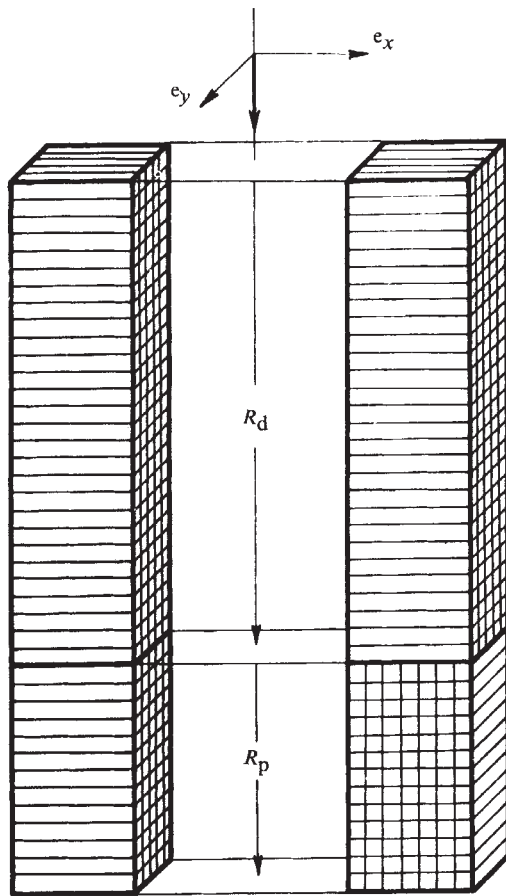


Fig. 1 The distal (R_d) and the proximal (R_p) rhabdomeres with parallel (left) or perpendicular (right) microvilli. Light propagates as indicated by thick arrow, thin arrows show the x - and y -orientations of the E vector.

On rotation of the E vector at an angle of 90° (y direction), transmitted light intensity will be given by $I_{yd} = I_0 \exp(-y_d)$ where $y_d = 2.3 \epsilon c_y l_d$, with c_y concentration of photopigment molecules orientated in the y direction, and $D = c_x/c_y$ is the dichroic ratio of a microvillus. The ratio of transmissions is

$$r_{td} = I_{yd}/I_{xd} = \exp(-y_d)/\exp(-x_d) \quad (1)$$

whereas the relation between absorptions which defines the PL response of the distal cell is

$$r_{ad} = [1 - \exp(-x_d)]/[1 - \exp(-y_d)] \quad (2)$$

As the same is true for the proximal rhabdomere, with $x_p = 2.3 \epsilon c_x l_p$ and $y_p = 2.3 \epsilon c_y l_p$ the relation between absorptions can be found for the proximal rhabdomere which, as suggested by this model, is responsible for the strong PL response of the whole compound eye.

$$r_{ap} = I_{yp}/I_{xp} = [\exp(-y_d)/\exp(-x_d)][(1 - \exp(-y_p))/(1 - \exp(-x_p))] \quad (3)$$

if the microvilli of both the rhabdomeres are parallel, and

$$r_{ap\perp} = [\exp(-y_d)/\exp(-x_d)][(1 - \exp(-x_p))/(1 - \exp(-y_p))] \quad (4)$$

if the mutual orientation of the microvilli is perpendicular, since for the proximal rhabdomere $c_{x\perp} = c_{y\parallel}$ and $c_{y\perp} = c_{x\parallel}$. Let the dichroic ratio $D = c_x/c_y$ of a single microvillus equal 2, as has been mentioned above, and the absorption coefficient be $1.7\% \mu\text{m}^{-1}$ as in crabs⁵ (that is a specific absorptivity of 0.017) when the E vector is parallel to the microvilli and $0.85\% \mu\text{m}^{-1}$ (or a specific absorptivity of 0.0085) for the perpendicular orientation of the E vector. For a distal rhabdomere $200 \mu\text{m}$ in length, $x_d = (0.017 \times 200) = 3.4$ and $y_d = 1.7$. For the proximal rhabdomere, which is usually shorter (say $70 \mu\text{m}$, near the

'optimal' value¹¹), $x_p = (0.017 \times 70) = 1.2$ and $y_p = 0.6$. Substituting these values in equations (2), (3) and (4), $r_{ad} = 1.18$ ('PL response' of the distal visual cell), $r_{ap\parallel} = 3.5$ and $r_{ap\perp} = 8.5$ ('PL response' of the proximal, or basal, visual cell). Of course, using these figures it should be remembered that the real electrical response of the cell is approximately proportional to the log of intensity absorbed.

Thus, a basal cell may seem responsible for acute PL perception in insects although its rhabdomere, as well as those of other visual cells, is built from relatively unsuitable material, that is microvilli having a dichroic ratio as small as 2.

The chief consequence of this consideration is that information on PL must be processed in a differential way. As the relation between the absorption of the distal and the proximal rhabdomeres does not depend on intensity I_0 (if bleaching is small), both the cells can make inputs of opposite polarities to the higher neurones which may be defined as a PL detector and which does not seem to show appreciable response to changes in intensity I_0 . On rotation of the E vector, however, the output signal of the distal cell remains nearly constant ($r_{ad} = 1.18$ in the above example) although the signal of the proximal receptor is greatly modulated by this rotation and this results, in turn, in a drastic change in the relation a_d/a_p between absorptions in distal and proximal rhabdomeres.

Two long axons coming off cells 7 and 8 in the fly retina bypass the intermediate retina and end in the medulla⁷. Such an organisation of nerve connections may be taken as morphological evidence in favour of the above differential mechanism. Unfortunately, nothing is known about the nerve connections of basal cells in the honeybee eye. It may be of interest that such a mechanism can provide a strong PL response by the eighth visual cell in the fly retina where the microvilli of cells 7 and 8 are perpendicular, maximal response being for the E vector perpendicular to the microvilli of cell 7. According to my morphological data⁸, in the honeybee retina the ninth cell has to show a smaller response to PL than the eighth cell of the fly because of the parallel orientation of the microvilli of the ninth cell relative to the microvilli of 'filter' rhabdomeres.

It follows from the model presented here that the ninth cell in the bee retina must be the only visual cell showing strong PL response. This prediction is in excellent agreement with recent findings⁹. The mechanism suggested is not at variance with the dielectric waveguide theory¹⁰ which, although complicated enough, must be developed for the rectangular dielectric waveguide⁸. More details on the 'filter mechanism' are given elsewhere¹¹. A deep theoretical analysis of the PL sensitivity of the insect visual cell has recently been carried out by Snyder¹².

F. G. GRIBAKIN

Sechenov Institute of Evolutionary
Physiology and Biochemistry,
Laboratory of Evolutionary Morphology,
Academy of Sciences of the USSR,
194223, Leningrad, Thorez pr. 52

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Alteration of Visual Cortex from Environmental Asymmetries

Wiesel and Hubel have shown that if normal simultaneous binocular vision is prevented during a period when an animal is particularly sensitive to the nature of its visual input, substantial changes can occur in the visual system¹⁻³. A dramatic extension of the influence of visual deprivation was made a few years ago. Kittens were reared viewing contours of a given orientation, and physiological studies showed that their visual cortical neurones had become tuned to the orientation they had experienced^{4,5}. In a related study, kittens were raised with prisms that caused vertical disparities between the images of the two eyes. Later it was found that the distribution of optimal disparities for which binocular cortical neurones were most responsive had undergone a shift, apparently to compensate for the rearing condition⁶. There is now an indication that the modifications can occur from an exceedingly brief selective exposure^{7,8} and recent behavioural data complement the original physiological finding⁹.

Experimental results with certain human subjects point to an analogous bias in early visual environment with subsequent neural consequences¹⁰⁻¹². The bias is introduced through ocular astigmatism, a condition in which the refractive power of the eye varies meridionally such that maximum and minimum powers are orthogonal. The effect is that detail of a particular orientation may be in focus while features of other orientations are blurred. So, a condition of preferential visual exposure obtains, but the astigmatic condition imposes a subtle, rather than an all or none, bias.

To see if partially selective visual exposure is sufficient to modify cortical neurones, we reared kittens with artificially induced astigmatism and then studied them physiologically. We found that the visual cortex of these animals was, in fact, decidedly abnormal. Cells in the cortex favoured the orientation which was in focus during rearing.

Our kittens were kept in a fully darkened room until they were 4 weeks old. For 3 h daily during the next 25 d, they wore masks containing cylindrical ophthalmic lenses. The remainder of their time was spent in the dark. The masks were positioned in total darkness so that the kittens' sole visual experience was obtained through the lenses. Two groups of kittens were studied. The first included a control animal whose mask contained lenses with no refractive power. Three other animals wore cylindrical lenses which limited clear focus to vertical, horizontal, and oblique contours, respectively. As the kittens were allowed to roam freely, they received a considerable amount of close-up visual input where the effect of the lenses was reduced. A more severe bias was arranged for the second group of kittens. Their lenses were of higher refractive power and their exposure period was spent on a clear Lucite platform so that the bulk of their visual input was from relatively distant areas where the lenses cause maximum asymmetries.

At the conclusion of the rearing period, the kittens were examined electrophysiologically. Fluothane was used for the initial anaesthesia. After tracheal and intravenous cannulation, anaesthesia was maintained with a nitrous oxide mixture. Standard procedures were employed to preserve good optics and to minimise eye movements. With tungsten-in-glass microelectrodes, we recorded extracellularly from single neurones in the primary visual cortex. The various stimulus parameters relevant to the cells' activities were explored with stimuli presented by an overhead projector and receptive fields were plotted. By use of an X-Y recorder, stimuli were presented manually and by computer commands. Altogether, we studied 408 units in six animals.

Cortical cells in the normal cat usually respond very specifically to stimulus orientation and our rearing conditions make orientation the feature of key interest. Results for the first group of kittens are shown in Fig. 1. Each histogram

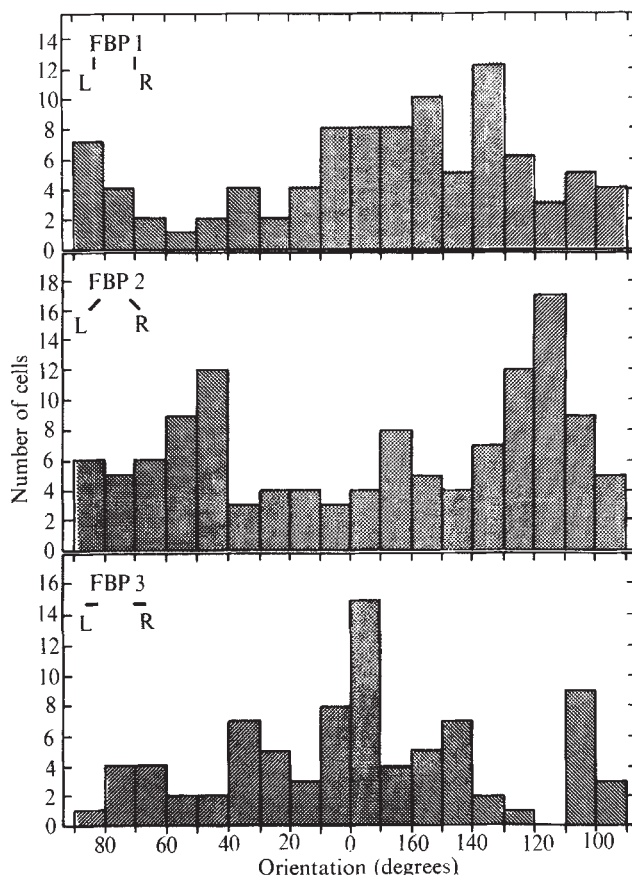


Fig. 1 Histograms showing the number of cells which respond best to a stimulus orientation within the 10° ranges indicated for three kittens. Each of the kittens has been reared with simulated astigmatism and the orientation which was in clear focus during rearing is indicated by the lines above the left (L) and right (R) eyes. Horizontal is 0° and vertical is 90°.

gives the total number of cells, both monocularly and binocularly driven, whose optimal stimulus orientation falls within a 10° range. 0° is horizontal and 90° is vertical. The insets indicate for each kitten the orientations which had been in clear focus during rearing. If the rearing conditions had influenced cortical neurones, the distribution of preferred orientations should peak at the clearly focused meridians. As shown in Fig. 1 the prediction holds for kittens FBP 2 and 3. For FBP 1, there is no skewing, but there is a substantial reduction in the number of binocularly driven cells as in the other astigmatic kittens (see below).

Figure 2 contains the results for the second group of kittens whose visual exposure was more strongly limited than the first group. Polar coordinates are used so that each line represents a cell whose preferred stimulus orientation is indicated by the angle of the line. For the normal cat, shown at the top, the distribution of preferred orientations is rather uniform and nearly all angles are represented. A marked departure from the normal pattern is seen in the astigmatically reared kitten. FBP 5, reared with oblique cylindrical lenses, received clear images of features with the indicated orientations. For each eye, the distribution of optimal orientations is clustered around the meridian which was in best focus during rearing, and the effect is most prominent in the left eye. A similarly asymmetrical orientation distribution is seen for FBP 6, and again a high density of preferred orientations occurs around the focused meridian, which in this case is horizontal. It is interesting to note that the right eyes of FBP 5 and FBP 6 show units for which the preferred orientations are nearly orthogonal to the meridian of clear focus. The factors most likely to be involved in this finding include accommodation and orientation

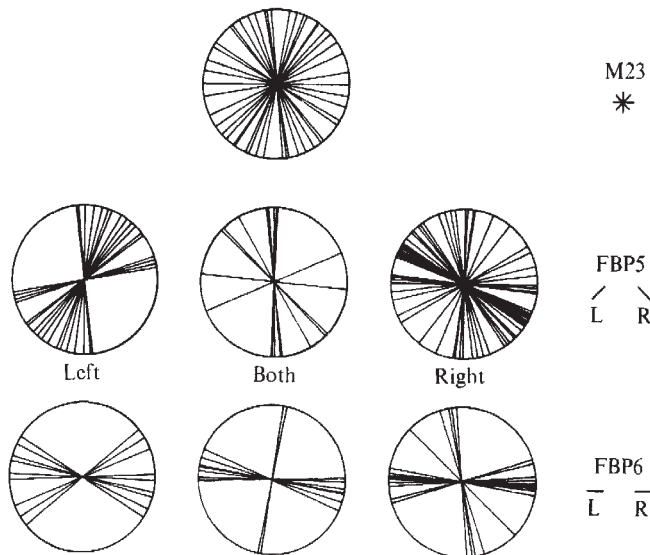


Fig. 2 Polar plots of preferred orientations for cortical cells are shown. Each line represents the optimal stimulus orientation for a single cell. The cat shown at the top is normal and below its name (M 23) several lines indicate that all orientations were present during rearing. The other cats have had a biased environmental upbringing caused by ocular astigmatism. As in Fig. 1, the lines above left (L) and right (R) eyes indicate the orientation which was in clear focus during rearing.

selectivity, as discussed in a detailed account of this work (Freeman and Pettigrew, unpublished).

Another finding, shown in Fig. 2, is the small number of units in the astigmatic kittens that are driven by both eyes. Only 25–35% of the cells are binocular as opposed to around 85% in the normal cat. One would expect a loss of binocularity in the case of oblique cylinders with different axes in front of each eye, but a reduction in the number of binocular cells also occurred with the bilaterally symmetrical biased kittens. The most likely explanation for the preponderance of monocular units is that the lenses were somewhat de-centred during rearing. Even a slight misalignment between the pupils and geometrical centres of the lenses would lead to considerable prismatic displacement.

The astigmatic rearing conditions also affected the limits of orientation selectivity. For normal cortical cells, stimulus orientation is usually very important, but the range over which the cell responds can vary considerably. Some cells are active over an angular region of only a few degrees while others respond over very wide ranges. In the astigmatic kittens, cells whose optimal orientation fell near the meridian of clear focus were also responsive over narrow to wide angular ranges as in normal kittens. But for the less frequently encountered units whose activity was highest when stimuli were presented away from the experienced meridian, we found that the response limits became broader. At a large angular distance from the clearly focused meridian, there were no finely tuned units. The implication is that a deficiency of features of a given orientation during early visual experience results in both a reduced number and a diminished selectivity of neurones which respond to that orientation.

Our results demonstrate that a subtle bias in the visual environment of a developing kitten is sufficient to markedly alter cortical physiology. The effects are graded in that our moderately biased kittens are modified less than the group for which bias was more severe. Thus, it appears that a kitten who has no previous visual experience is extremely sensitive to an environmental nuance. Even a relatively minor variation from normal exerts an influence on cortical connectivity.

It should, however, be mentioned that the type of approach

used in this investigation may be very useful in studying the physiological correlates of ocular refractive error.

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R. D. FREEMAN
J. D. PETTIGREW

School of Optometry, and
Departments of Physiology-Anatomy,
University of California,
Berkeley, California 94720

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Perception of Random Dot Interference Patterns

THE observation that circular patterns are immediately perceived if a random dot pattern (Fig. 1a) is superimposed on itself and rotated slightly (Fig. 2a) indicates that the visual system detects local autocorrelations and integrates these to form a global circular percept¹. By varying both the local and global parameters describing the interference patterns, the functional organisation of the visual system can be probed and new perceptual effects discovered.

There is nothing special about the circular morphology of the random dot interference patterns. By performing other transformations of the random pattern about a fixed centre point before superposition new morphologies arise. For example, by uniformly expanding Fig. 1a, we obtain Fig. 1b. A starlike pattern is produced when Figs 1a and 1b are superimposed (Fig. 2b). If they are superimposed in a rotated position, spirals will result (Fig. 2c). If Fig. 1a is expanded in one direction and contracted in the orthogonal direction (Fig. 1c) a hyperbolic pattern will result after they are superimposed (Fig. 2d). In these cases, for each point, $P_i(x_i, y_i)$, in the initial pattern, the transformations generate a new point, $P_i'(x_i', y_i')$, given by

$$\begin{aligned}x_i' &= ax_i \cos \theta - by_i \sin \theta \\y_i' &= ax_i \sin \theta + by_i \cos \theta\end{aligned}\quad (1)$$

where the x coordinate is multiplied by a , the y coordinate is multiplied by b , and the pattern is rotated through an angle of θ . Since this finite transformation can be thought of as an iteration of an infinitesimal transformation, each pair of correlated points can be thought of as lying on a single trajectory in a phase plane representation of two coupled, autonomous linear differential equations corresponding to the infinitesimal transformation. The dot patterns have the same morphologies as the trajectories of this underlying differential equation and can be predicted using the qualitative theory of differential equations². The eigenvalues, $\Gamma_{\pm}$, of the linear transformation of equation (1)

$$\Gamma_{\pm} = [(a+b) \cos \theta \pm ((a-b)^2 - (a+b)^2 \sin^2 \theta)^{1/2}] / 2 \quad (2)$$

are related to the eigenvalues of the underlying differential

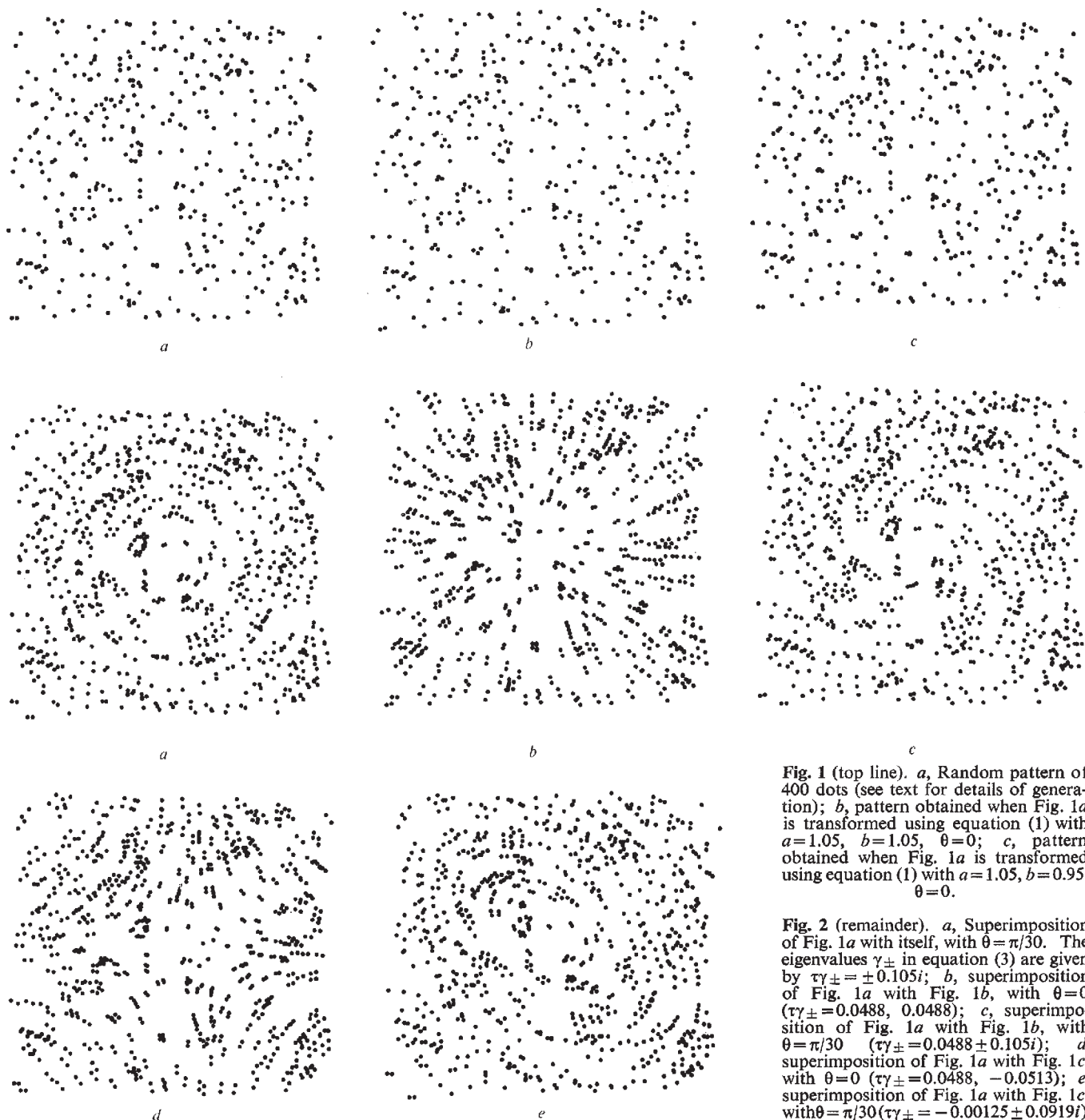


Fig. 1 (top line). *a*, Random pattern of 400 dots (see text for details of generation); *b*, pattern obtained when Fig. 1*a* is transformed using equation (1) with $a=1.05$, $b=1.05$, $\theta=0$; *c*, pattern obtained when Fig. 1*a* is transformed using equation (1) with $a=1.05$, $b=0.95$, $\theta=0$.

Fig. 2 (remainder). *a*, Superimposition of Fig. 1*a* with itself, with $\theta=\pi/30$. The eigenvalues $\gamma_{\pm}$ in equation (3) are given by $\tau\gamma_{\pm}=\pm 0.105i$; *b*, superimposition of Fig. 1*a* with Fig. 1*b*, with $\theta=0$ ($\tau\gamma_{\pm}=0.0488, 0.0488$); *c*, superimposition of Fig. 1*a* with Fig. 1*b*, with $\theta=\pi/30$ ($\tau\gamma_{\pm}=0.0488\pm 0.105i$); *d*, superimposition of Fig. 1*a* with Fig. 1*c*, with $\theta=0$ ($\tau\gamma_{\pm}=0.0488, -0.0513$); *e*, superimposition of Fig. 1*a* with Fig. 1*c*, with $\theta=\pi/30$ ($\tau\gamma_{\pm}=-0.00125\pm 0.0919i$).

equation, $\gamma_{\pm}$, by the relation³

$$\gamma_{\pm} = (1/\tau) \ln \Gamma_{\pm} \quad (3)$$

where τ is a non-negative, real constant

The dot interference patterns given in Figs 2*a-d* correspond to the only possible morphologies for two coupled, autonomous, first order, linear differential equations (or non-linear equations with non-zero linear terms) in the neighbourhood of a critical point and are called, respectively, a centre ($\gamma_{\pm}$ are pure imaginary), a node ($\gamma_{\pm}$ are real with the same sign), a focus ($\gamma_{\pm}$ are complex conjugates), and a saddle point ($\gamma_{\pm}$ are real with opposite signs)². If Figs 1*a* and 1*c* are superimposed and rotated the theory predicts that for $\theta > \sin^{-1} 0.05$, a focus will result, and this is observed (Fig. 2*e*). (For rotations in the range $2 \sin^{-1} 0.025 < \theta < \sin^{-1} 0.05$ there is a node, but as this comprises an angle of approximately $(0.025)^3$ rad or about 3 arc s it is not easily observed.) For any other differentiable transformation of the random dot pattern, the perceived patterns after superimposition of the original and its trans-

formed image correspond to the patterns generated by the trajectories of an underlying differential equation. This suggests that random dot interference patterns can be used to obtain qualitative information about the trajectories of first order differential equations of two variables. (The coordinates of each point in Fig. 1*a* were generated using a pseudorandom number generator (the RAND subroutine of FORTRAN IV implemented on an IBM 360 computer). The coordinates of the points in Figs 1*b* and 1*c* were then computed using equation (1). Figs 1*a-c* were plotted in a 6×6 inch area using a Calcomp plotter which printed an asterisk centred at each coordinate locus to ± 0.01 inch. To improve presentation, Figs 1*a-c* were drafted by hand from the computer printout. Transparencies of Figs 1*a-c* were made and these were superimposed to give Figs 2*a-e*. In the drafting process errors of approximately $\pm 1/16$ inch were introduced in the coordinate positions. By inspection of the interference patterns, in particular Figs 2*b* and 2*d*, these errors can be noted.)

The perception of random dot interference patterns requires integration of the correlated signals over a large portion of the visual field. This can be shown in a variety of ways. If all but a small region of the pattern (which is, however, large enough to contain several correlated pairs) is occluded, the structure of the unoccluded region can no longer be perceived. By selective attention the viewer will notice several local features which arise as a result of the random generation procedure, but which are generally overlooked when the global organisation is perceived. Calling the length of one side of Fig. 2a, L , it can be shown that for a point at a distance $3L/8$ from the centre (where the circular pattern is readily seen), the number of uncorrelated points lying closer to a given point than the correlated point is approximately four. This shows that information from several pairs of correlated points must be integrated to detect the local correlations.

Transparent negatives of the patterns in Fig. 1 were made and correlated pairs were mounted so that when the images were projected using a stereoscopic projector and viewed without polaroid glasses, the negative images of the patterns in Fig. 2 could be seen. The intensities of each dot pattern could then be varied independently by rotating polarizers in the optical path. Consider the superimposition of Fig. 1a with itself, where the two patterns are labelled P and P' , with P' rotated clockwise with respect to P ; as the intensity of P' decreases with respect to P the entire figure appears to rotate in an anticlockwise direction; as the intensity of P' increases with respect to P a clockwise rotation is seen. Analogous results occur from the superimposition of the other pairs of figures used to construct Fig. 2. For example, calling Figs 1a and 1b, P and P' , respectively, we find that decrease of intensity of P' results in a perceived contraction of the figure, while increase of the intensity of P' leads to a perceived expansion of the figure. For each case, although there is no real motion, there is a perceived apparent motion which can be described by saying that we perceive the movement of the centre of intensity of each pair of correlated points. Partial changes of intensity lead to partial motions. Although the perceived apparent motion is similar to the familiar "phi phenomenon"⁴, important differences distinguish the two effects. First, we have unsuccessfully attempted to observe partial motion as a result of incomplete intensity changes for the phi phenomenon, and have also been unable to locate any other reports in the literature of partial motion for this effect. Second, the motion effect described here requires a large region of correlated pairs of dots. If only a small portion of a pattern is presented, the effect disappears, mimicking the disappearance of perceived patterns of Fig. 2 under presentation of a small portion of the pattern. Over a wide range of intensity differences of the two subpatterns, the patterns of Fig. 2 can still be perceived. At very low intensities of one pattern with respect to the other, the patterns of Fig. 2 can, however, no longer be perceived and a random pattern is observed. The motion effect is particularly striking using the more easily generated splatter patterns presented in ref. 1.

The colours of each of the subpatterns of Fig. 2 can also be changed independently by interposing coloured filters in the optical path. The effects previously described remain unchanged. The patterns of Fig. 2 as well as the apparent motion effect can still be observed when the two subpatterns are of different colours. In addition, the resulting patterns give a striking presentation of the colour stereo effect⁵⁻⁷, in which different colours appear in different depth planes as a consequence of chromatic aberration of the eye; the effect is strongest with red and blue filters and low light intensities.

The patterns of Fig. 1 were presented binocularly as random dot stereograms (Julesz patterns)⁸. Negative slides for use with the stereoscopic projector, as well as positives which can be fused directly (or by using a stereoscopic viewer), were prepared. Initial experiments were performed using a random pattern and its rotated image. Using this stimulus it is possible that torsional peripheral movement of the eyeball could bring

both subpatterns into correspondence. To eliminate this possibility slides were prepared with vertical and horizontal axes passing through the centre of both the original and its rotated image. When these slides were projected and viewed without polarising stereo viewers, a circular pattern intersected by a single set of orthogonal axes is observed. Upon binocular presentation (with polarising glasses) the circular pattern disappears. The coordinate axes are fused, and with stereopsis a single set of random points are perceived lying on a tilted plane passing through the horizontal axis. No peripheral eye movements can bring the other subpatterns, presented as stereo pairs, into correspondence. Stereopsis can be readily performed with Figs 1a and 1b and also with Figs 1a and 1c. In each case, the interference patterns of Fig. 2 are destroyed and a single pattern of dots seen in depth is always perceived. In binocular presentation of random dot stereograms, cross correlations between the images in the eyes are computed with any disparities leading to depth perception. (See ref. 8, especially Figs 2.8-8, 6.5-3 and 6.5-4 and related discussions.) In binocular fusion of the patterns the small errors introduced by hand drafting are particularly noticeable and can be seen as small deviations away from the smooth surfaces defined by the disparities.

A major problem facing neurophysiologists is the determination of the physiological basis of form perception in higher animals. Major steps forward have been made in elegant demonstrations of cortical units which respond to a variety of local features in the visual field: stationary oriented stimuli⁹, moving oriented stimuli^{9,10}, and disparities between oriented stimuli in the right and left eyes^{11,12}. Although the demonstration of local feature extractors at the cortical level is suggestive, this demonstration is in itself insufficient to show the involvement of these units in perception processes requiring integration of local features to form global percepts. Further, few data are available concerning neutral units sensitive to global parameters of the visual input. The random dot interference patterns and stereograms provide uniquely simple stimuli in which local correlations must be integrated to form global percepts and in which both local and global features of the patterns can be independently varied. The use of these patterns as input stimuli in physiological experiments seems justified.

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LEON GLASS
RAFAEL PÉREZ *

*Institute for Fundamental Studies,
Department of Physics and Astronomy,
The University of Rochester,
Rochester, New York 14627*

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* Permanent address: Instituto de Física, Universidad Nacional de México.

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¹² Nikara, T., Bishop, P. D., and Pettigrew, J. D., *Expl Brain Res.*, **6**, 353 (1968).

BOOK REVIEWS

Why a Textbook?

The Special Theory of Relativity. By H. Muirhead. Pp. xi+163. (Macmillan: London and Basingstoke, September 1973.) £3.95 boards; £2.45 paper.

DR MUIRHEAD describes this as "a textbook on special relativity from the high-energy physicist's point of view". He expresses surprise that "no undergraduate textbooks on special relativity have been written by high-energy nuclear physicists". Although his own attempt is enterprising and courageous, I hope it is not too unkind to suggest that it rather demonstrates why this should be the case. For the problems of high-energy physics for which special relativity concepts are important are problems for which much more difficult quantum concepts are even more characteristically important. By the time a student is ready to tackle the quantum ideas, he ought to be able to use special relativity almost without thinking. To attempt to teach special relativity to undergraduates with emphasis on applications to high-energy physics, with all respect to an expert in such physics, does appear to be doing things the wrong way round. The attempt inevitably results in the need to say of one topic that it "can only be properly appreciated within the framework of quantum mechanics", of another that "the story is complicated and we shall not pursue it here", and so on. Thus at best the student can only half understand what he is reading about. Actually Dr Muirhead does his best to cope with this situation by providing a set of carefully chosen "problems" at the end of each chapter, so that the student can at any rate test his understanding. And, of course, not every application discussed has a bias towards high-energy physics; for instance section 3.6, "The visual appearance of rapidly moving bodies", is an interesting simple topic that happens not to find a place in most textbooks and it seems to have inspired the publisher's attractive jacket design.

As a text on special relativity, a reviewer is bound to remark upon some shortcomings. There is no clear pattern to the mathematics; some deals with space and 3-vectors together with time, some with space-time and 4-vectors, and there is fragmentary use of matrices and tensors. Some of the derivations are laboured and confusing, as for example that of the Lorentz force in section 6.5. While the printing is clear, the layout is in some respects curiously inelegant,

and there are numerous misprints—on page 125 a publishing house is called →ergamon →ress. A book of this sort makes one wonder why any textbook on special relativity is still needed. For special relativity is not a department of physics but a basis for nearly all physics. Almost everything that is known about it has been known for nearly seventy years; so one would think that by now it ought to have been absorbed into the general formulation of physics as a whole. How is it, then, that students still have to learn special relativity much as their predecessors have done over the past seventy years? They first become imbued with all the ideas of classical physics, and afterwards some of them acquire some small text that conveys to them how certain of these ideas may be somewhat unsatisfactory although this may be unimportant unless they have to play around with speeds approaching light-speed. And nearly every such text, that under review being no exception, starts with the same sort of historical introduction that has been the fashion from the start. So the reader's mind is conditioned to find relativity surprising and not quite "natural".

A simple partial answer is, naturally, that non-relativistic physics is so successful for most of what interests most people that these people have no practical need for anything else. And the comparatively few who do need relativity for practical purposes can get along by learning to use "relativistic equations of motion" without troubling about the rest of the subject.

To go a little deeper, while relativistic electromagnetism can be made simpler than non-relativistic, relativistic mechanics is much less simple than non-relativistic and, of course, the electromagnetic field may be observed only through its mechanical effects. Since then relativistic mechanics is not included in most elementary treatments of mechanics, one must rather grudgingly admit that a textbook of special relativity is still needed. Only let it be of some logical form and mathematical elegance, and without a historical introduction!

Maybe the best preparation for learning special relativity would be provided if someone nurtured upon relativity would write a textbook of general classical physics. He could have fun formulating postulates on rigid bodies, clocks, electromagnetic fields, and so on, so as to construct a self-consistent system yielding all pre-relativity physics—even including the paradox that if two twins separate and meet again after

completely different careers their ages must nevertheless be the same. Most readers would then turn with relief to relativity physics.

W. H. MCCREA

Brainwaves

Explorers of the Brain. By Leonard A. Stevens. Pp. 330 (14 plates). (Angus and Robertson: London and Sydney, 1973.) £2.95.

I FIND this book very attractive. It is a most readable and lively text enriched by a wealth of stories and anecdotes that are surprisingly accurate in all the fields in which I can check them. The author has made a remarkably erudite study of the history of brain exploration from Greek times to the present. He has travelled very widely in the United States, visiting a large number of laboratories which he lists in the acknowledgments section. Understandably his writing is coloured by the experiences he has had and the advice he has been given in these various centres for brain research. Nevertheless when he is dealing with important work in other countries, he is also very well informed. I am able to judge this in some fields in which I have personal experience as, for example, the Loewi-Dale story of chemical transmission and the Hodgkin-Huxley-Katz-Cole-Curtis story of the action potentials of giant axons. He gives a very well informed account of the EEG and the role of Hans Burger in this remarkable discovery.

Of particular value is his account of Ramón y Cajal whom he quite rightly makes one of the heroic explorers of the story. As in the case of Ramón y Cajal the chapter often starts with some remarkable event vividly told, and then it runs through a flashback of the scientific discoveries that led to this initial vivid scene. In this dramatic way, he is able to hold the interest of the reader.

Of course, in such a wide field as brain research, there must be many omissions of distinguished investigators. For example, there is no mention of Lucas, Zotterman, Mountcastle, Hubel, Wiesel, or Kuffler. In part these omissions arise because the very important afferent side of nervous pathways has been virtually ignored. On the other hand, there are superficially exciting scientific claims that have an undue treatment in the text. No doubt the author feels that these stories are valuable in his efforts to stress the play and interplay of scientific ideas in the growing fringes of brain investigations.

These are of course minor criticisms, and I would like to conclude by recommending the book very widely to all students of the brain and to all those interested in the history of the remarkable discoveries that have been made in this field: I have myself recommended it to my research associates. It is too often a temptation to the young investigator to narrow his field of interest unduly. A book such as *Explorers of the Brain* will open to him most attractive vistas in this immensely exciting field.

J. C. ECCLES

Clouds of Glory

Early Human Development. Edited by S. J. Hutt and Corinne Hutt. Pp. viii+362. (Oxford University: London; Clarendon: Oxford, June 1973. £2.75 boards; £1.50 paper.

THIS book should be called "Readings in Early Human Development". It reprints twenty-nine papers (some mercifully shortened), grouped in seven sections. Each section has a brief but well documented introduction. Most of the papers are without summaries. There is a perfunctory subject index, but no index of authors.

The first section, on genetical influences on behaviour, begins well. The editors give as the important general questions, "How much of the variance of scores on a particular trait under certain environmental conditions can be attributed to genetic factors? How modifiable is the phenotypic expression of such a trait? How do genetic and environmental influences interact?" Unfortunately, in the introduction to section 5, they drift into the usual confusion between genetical and ontogenetic determination; but this is only a minor lapse.

After the genetical section, three papers deal with hormones and behaviour. The third describes some unexpected findings which suggest that dosing a pregnant woman with progesterone can accelerate the early physical development of her child and can later enhance his intellectual development. The next section, entitled "Early Brain Mechanisms", is inevitably gravelled for lack of matter. It includes a study of rhythms of sleep and waking, and one on the effect of cycles of internal state on responsiveness to external stimulation.

Perception is covered by three papers on vision and one on auditory discrimination. The recent important work on the effects of errors of refraction on the development of vision was perhaps published too late for inclusion.

Two sections, under the headings of "Attachment" and "Early Experience", include papers on maternal deprivation. A classical paper by John Bowlby, published 21 years ago, is rightly in-

cluded; and there are more recent and more critical studies in the same field, notably one by H. R. Schaffer entitled "Maternal Deprivation or Perceptual Deprivation?". Another paper discusses the reversibility of the effects of early lack of mothering. But some important investigations, which attempt to relate the precise nature of the deprivation to subsequent development, are not represented.

Finally there are six papers on stimulation in early life and on the development of fear. These illustrate, among other things, the importance of this topic and how much remains to be learnt about it.

This is a useful collection. It includes papers which illustrate some of the many methodological hazards of work in this field. It would have been still more useful if the editors had written more, and so filled some of the gaps. Among special but important items not adequately covered are researches on sucking and — still more notable — investigations of intellectual development in the first year. The editors seem to have some doubts about the usefulness of cross-cultural studies, and they are not represented. These include findings, on the influence of social class on development, which ought to influence social policy. But if, as the editors rightly assume, comparisons with other species can suggest testable hypotheses about man, then comparisons of human groups can do so too.

Despite omissions, some of them no doubt inevitable, this volume gives a helpful and instructive picture of a region of study which is now full of life and could hardly be more important.

S. A. BARNETT

The Airport Affair

A Sadly Mismanaged Affair: A Political History of the Third London Airport. By David McKie. Pp. 256. (Croom Helm: London, October 1973.) £3.75.

DAVID MCKIE, of *The Guardian*, tells a lively tale of the political problems of deciding where London's third airport should go. In 1944 Lord Swinton, Minister of Civil Aviation, had been able to say of Heathrow that it was the "only possible site" for London's airport. No one is so sure about anything in 1973. The book takes us briskly through Croydon, Heathrow, Gatwick, Stansted, Cumblyington, Thurleigh, Nuttampstead and Foulness, and it is clear, after the inquiries and committees, that it is possible to prove anything once the terms of reference are designed properly. Even so, an airport seems to be one monument to which politicians don't crave to have their names attached.

Roskill is viewed with some awe and,

it must be said, doesn't come over very clearly. One senses that Mr McKie has not really tried to understand the technical side of the issues at all, as is his privilege in what he calls a political history. Nevertheless, this does make the book somewhat unsatisfactory. There are no figures on the predictions and realities of usage of airports, on the impact of wide-bodied jets on airport planning, on the levels of noise at varying radii from airports, on the improved noise performance of modern engines and on many other technical issues. The reason that the planning process has been so tortuous is not simply that there have been local objections; the technical ground has been shifting continuously under the politicians' feet. The book is incomplete, however readable, without the driving forces being in clearer focus.

D. DAVIES

Plasmids

Bacterial Plasmids: Conjugation, Colicinogeny and Transmissible Drug-Resistance. By G. G. Meynell. Pp. xiii+164. (MIT: Cambridge, May 1973.) \$14.99.

DURING the past decade there has been a considerable expansion in the study of bacterial plasmids. From the early days, when the plasmid F served as a mere tool for unravelling the intricacies of *Escherichia coli* genetics, their importance has increased concomitant with the realisation that quite unrelated plasmids are of relatively frequent occurrence in many bacterial genera, and that in many cases they have the ability to cross interspecies and intergeneric boundaries. Combined with their ability to carry genes determining resistance of the host to antibiotics, these properties give them great medical importance, while their ability to transfer host chromosomal genes suggests that they may play a significant role in the evolution of bacterial species.

There will therefore be wide interest in this book, which sets out to review the important biological characteristics of bacterial plasmids. The three central chapters (chapters 5, 6 and 7) deal with their transmission from cell to cell by conjugation, their replication within the cell as autonomous DNA molecules, and the physical properties of these molecules. These chapters provide a brief but adequate introduction to plasmid biology, although, inevitably for a book written two years ago about a rapidly-expanding field, much important recent information is absent. In particular, the chapter on conjugation is noticeably out of date, and topics such as the discovery of a large number of plasmid incompatibility groups, and the use of electron microscope heteroduplex tech-

niques for physical studies of plasmid molecules, are necessarily but unfortunately missing.

The discussion of plasmid-host chromosome interactions in chapter 3 is less satisfactory, because of its heavy dependence upon analogies with λ phage—which I consider to obscure, rather than clarify, the properties of transmissible plasmids. In other places, the text could have been lengthened with advantage; for example, the accounts of plasmid-determined characters in chapter 7 are really too short to be useful, and surprisingly little attention is paid throughout to plasmids other than those found in the Enterobacteriaceae.

Despite omissions and occasional obscurities, the book provides an introduction to the biological properties of bacterial plasmids, and an extensive bibliography, that medical and science students will find of value. The requirements of the research worker, however, will be better satisfied by the recent spate of review articles.

N. S. WILLETT

Hydra

Biology of Hydra. Edited by Allison L. Burnett. Pp. xv+466. (Academic Press: New York and London, May 1973.) \$29.

THE title of this book gives the impression that its scope is wider than in fact is the case. Of the sixteen chapters, twelve are concerned with developmental topics, including discussions of the developmental significance of interstitial cells, organismic polarity, histological and ultrastructural studies of the development of nerve cells and of cyto-differentiation during regeneration, cell proliferation in relation to form and the structure and developmental significance of the mesogloea. The remaining chapters are devoted to behaviour, feeding and digestion, ecology and neuro-secretory cells. The emphasis on developmental studies is a fair reflection of the distribution of research activity in recent years.

Although it is nowhere made explicit, all the contributors (with one possible exception) are present or former students and colleagues of the editor; this gives the book a somewhat parochial air. The contributions are rather a mixed bag. Among the more useful I would single out Rushforth's introductory survey of behavioural studies which is clearly written and informative; and Diehl's discussion of interstitial cells, which I hope will at last put an end to the arguments about the morphogenetic role of these cells (they have none) which have plagued the literature for so many years.

Some of the other contributions on developmental topics are of more doubt-

ful value and I would single out two for specific comment. The chapter on organismic polarity by Lesh-Laurie seems to me quite inadequate. This is an area in which significant progress has been made in recent years and general models have been developed which are applicable not only to *Hydra* but also to many other developing systems; in this respect there has been a genuine, if incomplete, synthesis. No one reading this review would gain any hint of these advances, since the relevant literature is not cited. Admittedly, the most comprehensive discussions have appeared relatively recently, but Wolpert's theoretical discussion which appeared in 1969 is not quoted and no reference is made to the important experimental work of Rose on *Tubularia* which is now 16 years old.

Whereas I find the discussion of polarity merely inadequate, I must say that I regard the chapter devoted to cell proliferation in relation to morphogenesis as positively confusing and misleading. The points at issue are whether patterns of cell movement in the growing organism are to be explained in terms of patterns of cell production or in terms of patterns of cell loss and whether morphogenesis is to be explained in terms of localised cell proliferation or in other terms. The axial pattern of cell proliferation, so stressed in this chapter, is an important piece of evidence in this argument but by no means as decisive as the editor and author imply. The whole subject is treated in such a way that the uninitiated might get the impression that the ideas of other workers in the field—particularly those of Campbell, whose evidence and arguments are inadequately discussed—have been refuted. This is certainly not the case; Campbell's work remains as compelling as ever and the new data which Dr Corff presents at such length do not seem to me to constitute evidence which can be used one way or the other in this argument.

I do not think this book can be regarded as anything more than a convenient, if extremely expensive, summary of the work of the Burnett "school". The greater part of it is concerned with developmental problems but it is both too parochial and too defective in key areas to constitute a definitive survey of our present knowledge in this field. In terms of theoretical content the book is very weak; no formal models are discussed, though there are several in existence, and such theory as there is consists for the most part of vague speculation. In too many chapters the authors are content to present the reader with a mass of experiments and observations and leave him to draw his own conclusions as to its possible meaning.

GERALD WEBSTER

Jobs for Graduates

Patterns of Change in Graduate Employment. Edited by H. Greenaway and G. Williams. Pp. 171. (Society for Research into Higher Education: London, September 1973.) £2.70.

"DESPITE the growing concern about the employment of graduates", say the editors of this collection of essays by different authors, "there has so far been little attempt to place the problem in perspective". Certainly much of the public discussion has been concentrated upon short-term variations in the pattern of supply and demand, particularly for newly-qualifying scientists and engineers. The difficulties of those in mid-career have received scant notice, and one of the best parts of the book is the chapter on this subject by W. G. H. Robins, who shows how relatively large this problem may be.

The nation is, however, committed to the continued expansion of higher education with over 20% of eighteen-year-olds, and over 25% of eighteen-year-old boys, entering higher education by 1981; these students will then be asking for and expecting jobs appropriate to their level of education. It is idle to pretend and dangerous to assume that this can happen without stresses affecting both the newly qualified and employers and their existing staffs, or that economic growth can, by itself, create sufficient jobs at the right level. A problem exists of a continuing nature proportional in size to the rate of change of the supply and demand situation, or more accurately to the value of the second differential.

In discussions of supply and demand, it should not be forgotten that here the supply is of human beings, with hopes, prejudices and fears, and Tom Snow writes perceptively about students and their problems in choosing their first occupations. There are other good things in the book, such as E. G. Whybrew's brief chapter and Gareth Williams's illuminating analysis of the economics of the graduate labour market, but some of the most disappointing parts are those for which the editors have been personally responsible. Their joint final essay "Is There a Crisis?" is unexceptionable but their early individual chapters suffer from obscurity, inaccuracy and lack of perspective. The roots of today's manpower situation lie deeper in the past than the Robbins Report of 1963; arguably for scientists and technologists they derive from the connection between fundamental and applied science and national security, and the line from 1940 to 1970 through the Manhattan project to the Apollo project is continuous. There has also been the belief that more research meant more economic growth, a belief which Robbins institutionalised

at a moment when it was no longer true. Harriet Greenaway's "Impact of Educational Policies" starts with Robbins and concentrates mainly on the past five or six years, with too much attention devoted to very recent happenings, some of them unimportant. First employment statistics are discussed by both editors separately in chapters 1 and 2; the reason for this is unclear, the tables are confusing and the total effect is more muddled than even this difficult subject need be. In spite of these defects this is a worthwhile book, and there is a useful six-page bibliography.

B. J. HOLLOWAY

Ectomycorrhizae

Ectomycorrhizae. Edited by G. C. Marks and T. T. Kozlowski. (Physiological Ecology: a Series of Monographs, Texts, and Treatises.) Pp. xiv + 444. (Academic: New York and London, June 1973.) \$28.50.

THE content of this latest volume on mycorrhizae, specifically ectomycorrhizae in this instance, reflects a revival of emphasis on the general biology of these structures. The emphasis is very much on the need to relate more carefully findings from controlled laboratory experiments to complex soil situations.

Many references are made to ectomycorrhizal synthesis. Well tried aseptic techniques are described and at the same time mention is made of the need to consider possible role(s) of rhizosphere microorganisms in synthesis and function. The inclusion of a specific rhizosphere chapter (by Rambelli) is excellent, but the chapter itself is a little disappointing both in presentation and in the errors it contains. The section on the concept of the rhizosphere is muddled, and the data of Buxton (1957) on page 323, Slankis *et al.* (1964) on page 318 and Rovira (1965) on page 314 are either incorrectly quoted or presented in a confused way. Also Figs 1 to 8 do not relate properly to the accompanying text.

Bjorkman's hypothesis, it is concluded by various contributors, is an oversimplification of the situation. The comments of Slankis on the probable involvement of growth hormones are very relevant and the concept of a mycorrhizal infection zone (MIZ), as outlined by Marks and Foster, should prove of value in further studies on the synthesis problem. The comments of Bowen and Theodorou on the need to separate the process of root surface colonisation from that of infection proper involving the formation of an Hartig net are also important.

Ectomycorrhizal function is also discussed at length. Bowen's contribution on mineral nutrition is quite excellent and his remarks on the need to study

soil situations such as form and mobility of ions in soils and the importance of mycelial strands are very logical. This author also mentions field inoculations and forestry practice. This more applied aspect of mycorrhizal studies is further developed by both Meyer and Mikola in their respective chapters. The possibility of conducting field inoculations with fungi suited to particular situations such as soil types and climate, and the necessity (or not) of raising nursery seedlings under conditions conducive to mycorrhizal infection, are discussed. Somewhat related to these points, perhaps in connection with the establishment of culture collections of useful ectomycorrhizal fungi, are the problems of their precise identification as discussed by Zak.

Marx deals with the function of ectomycorrhizae as protective agents against certain soil pathogens. Several contributors venture the opinion that nutrient uptake and protection against soil pathogens are the two important functions of ectomycorrhizae but that there are others.

The importance of nitrogen in ectomycorrhizal synthesis and function is mentioned many times but it is clear that much work still remains to be done on its general involvement. Its relative importance to phosphate is still not known. The same sort of comments are relevant to studies on the involvement of root exudates (and metabolites). Here it seems that too much emphasis has been put on aseptic laboratory experiments.

It is something of a relief to see the consistent use of the term ectomycorrhiza(e) throughout the book. I am less happy about the use of the terms obligate and facultative ectomycorrhizal tree as proposed by Meyer. Applying these terms ecologically, as he suggests, is virtually impossible.

Despite the inevitable variable quality of the chapters, most are excellent. The volume as a whole is well integrated and makes for consistently stimulating reading. It certainly rates as a significant contribution to the study of ectomycorrhizae.

B. W. FERRY

Decision and Reaction

Attention and Performance IV. Edited by Sylvan Kornblum. Pp. xxxvii + 771. (Academic: New York and London, April 1973.) \$18.50.

THE series of volumes with this title forms one of the curious spontaneous developments of modern psychology. The first appeared six years ago; it reported the papers given at a small gathering of like-minded researchers organised in Holland by Andries Sanders. The gathering was fun; two more meetings followed in quick succession, and each gave rise to its volume of

proceedings. Usually, volumes assembled from conference papers are uneven, badly edited, and packed with the output of those who need to give a paper to get their fare; but *Attention and Performance* was not like that. The three volumes have been eagerly read by advanced workers and students alike, and contain many of the basic papers in the newly expanding area of cognitive psychology. Now the fourth volume appears; this time the conference moved from Holland to the USA, the editor and publisher ceased to be Dutch, and the sheer thickness of the volume has increased. The question obviously arises, is the magic still there?

Briefly, it is. There are forty papers in the volume, and almost every one makes some point which needs consideration even if one disagrees with it. No journal has such a high success rate, and once again the book should be bought by anybody who wants to know what is happening at the frontiers of research into the areas covered. This time, the sections of the meeting were devoted to selective attention, physiological correlates of attention, sequential effects in reaction time and judgment, organisation of memory, and judgments of temporal order. There are also sections on two special aspects of reaction time, which are titled "binary classification" and "choice" respectively, but which might be better described as "experiments in which a person decides whether two patterns are the same or different" and "the relation between speed and accuracy in decision".

These areas are technical, and this is not a book for the amateur. In an attempt to widen its scope, each section starts with a "tutorial review" of the area, but it was perhaps too optimistic to expect first-rank workers to forget that at the meeting itself they were talking to the highly knowledgeable. The best review for outsiders is that by Vaughan and Ritter on the various cortical evoked potentials and their relationship to behaviour; runners-up are Kornblum on sequential effects and Nickerson on the "binary classification" task, provided one already knows why these topics are important. Other reviewers such as Moray and Fitter, Norman, or Audley used the occasion to put forward a general and highly original framework of their own for theorising in the particular area, and these might be positively misleading for the unguided novice. This departure therefore did not succeed in making the book into a true textbook. But was such an attempt worth while? The real virtue of *Attention and Performance* lies in the opportunity it gives for some of the brightest minds in psychology to strike sparks off each other at a technical level. Long may it continue.

DONALD E. BROADBENT

Obituary

Selman Abraham Waksman

IN Woods Hole, Massachusetts, on August 16, 1973, the eminent Nobel laureate microbiologist, Selman A. Waksman, died suddenly of a cerebral haemorrhage. There, three days later, he was buried far from his native Russia but close to one of the laboratories he had helped to develop.

Selman Abraham Waksman was born on July 22, 1888, in the town of Priluka near Kiev in the Ukraine. Finding it difficult, as a Jew, to enter the University of Odessa, he left for the United States in 1910. The next year Waksman entered Rutgers College (New Brunswick, New Jersey) on a State scholarship. He received a Bachelor of Science in Agriculture in 1915 and was elected to Phi Beta Kappa, the first of the many academic honours his incredible intellectual industry would yield. The next year, at the same institution, Waksman received a Master of Science degree for work done under the supervision of Dr Jacob G. Lipman. In 1918 he received a PhD in biochemistry from the University of California in Berkeley. Except for part-time appointments at Cutter Laboratories in California and at Takamine Laboratories in New Jersey, Dr Waksman's whole career developed at Rutgers University where he was first appointed in 1918, and promoted to Professor in 1930. In 1940 he was named head of the Department of Microbiology of the College of Agriculture, and in 1949 he became Director and founder of the Institute of Microbiology of Rutgers University. He retired in 1958 but continued for many years to direct a limited amount of research.

Dr Waksman was a prolific writer. During his life he and his collaborators published more than 500 papers and twenty-eight books. This output ranged from booklets giving practical advice on the use of enzymes for such purposes as jelly-making, soil fertilisation, and mushroom growing, to erudite historical studies. Between these two extremes stands a mass of research communications centring around soil microbiology.

While still a student at Rutgers, Dr Waksman became interested in the ecology and the taxonomy of actinomycetes. At first, he thought of these organisms merely as decomposers of organic matter playing a key role in the building of humus and the modelling of the soil. However, in 1923, he and Robert L. Starkey wrote in *Soil Science*: "Certain actinomycetes produce substances toxic to bacteria, as shown by

Greig-Smith (in 1917) and as often observed in our work, when around an actinomyces colony upon a plate, a zone is found free from fungus and bacterial growth."



In 1937, in collaboration with Jackson W. Foster and Imri J. Hutchings, he published in the same journal a series of papers on associative and antagonistic effects of microorganisms. Following the spectacular success of his former student, René J. Dubos, who discovered tyrothricin in 1939, as well as the revival of Alexander Fleming's penicillin by Ernst B. Chain, Howard Florey and associates in 1940, Dr Waksman concentrated on the study of antibiotics. A total of twenty-two antibiotics, mainly from actinomycetes, were isolated in his laboratory, four of which have been useful enough to be produced commercially for medical use. These are actinomycin, an antitumour agent, streptomycin and neomycin, active against bacteria and mycobacteria, and candicidin, an antifungal agent. Of these, streptomycin, isolated in 1944, is the most widely employed and its use revolutionised the treatment of tuberculosis and closed many a sanatorium. For the discovery of this compound, Dr Waksman was awarded the Nobel Prize in Physiology and Medicine in 1952.

Before becoming involved in the study of antibiotics, Dr Waksman concentrated on the role of microbes in the soil and in the building of humus. He was in turn an expert on microbial enzymes, autotrophic bacteria, especially sulphur bacteria, the formation and the use of peat, marine microbiology, the production of organic acids by fungi, the role of thermophilic organisms in composting, the taxonomy of actinomycetes, the role of heavy metals in the nutrition of fungi and the history of microbiology. In this last field, his

major contributions were biographies of Haffkine, Winogradsky and Lipman.

Dr Waksman organised the division of Marine Bacteriology at Woods Hole Oceanographic Institution in 1931 and for over a decade he contributed to our knowledge of the ecology of marine microbes. In the sea, as in the soil, he was concerned with the role of microorganisms in the decomposition of organic matter. He recognised that microbial activity decreased with an increase in depth. He considered fungi and actinomycetes to be soil organisms which may be only casually carried to the sea from their natural habitat on land. Upon his retirement from active research at Woods Hole Oceanographic Institution in 1942, he was elected a trustee and he served in this capacity until his death.

The bulk of the royalties received from the manufacturers of streptomycin and neomycin was used to establish an Institute of Microbiology which opened its doors in 1954. The original plan of Dr Waksman was to explore six major fields of research: general microbiology, microbial physiology, antibiotics, vitamins and enzymes, microbial ecology and applied microbiology. As the Institute developed, certain fields such as the study of vitamins were neglected while others were added. Among these were immunochemistry under the leadership of Michael Heidelberger, as well as other fields such as genetics and virology.

Dr Waksman was a link between microbiologists of the United States and of the Soviet Union. He wrote numerous articles on Russian scientists and their work and visited the Soviet Union several times.

Dr Waksman was of Jewish stock, and although not a religious man he was a keen scholar of Jewish history and institutions. He visited Israel on several occasions and played a key role in the creation of the Institute of General and Industrial Microbiology of the Technion of Haifa.

A short, stocky man with a perennially busy air, Dr Waksman loved to recount stories about his life and career. They were always pertinent to the discussion or lecture of the moment, and usually provided an historical or scientific insight for his listener. He was a loyal supporter of all those who had gained his confidence and who now mourn a departed friend.

Dr Waksman is survived by his widow, Deborah B. (Mitnik) Waksman, and his son Dr Byron H. Waksman, a distinguished immunologist who is Professor of Microbiology at the Medical School of Yale University.

Announcements

University News

F. Moreton and L. R. G. Treloar have been appointed Professor Emeritus by the University of Manchester.

Miscellaneous

Sir Martin Ryle, Professor R. R. Porter and Professor E. P. Abraham have been awarded the three Royal Medals of the Royal Society.

Erratum

In the article "Geomorphological Dating of Cave Opening at Makapansgat, Sterkfontein, Swartkrans and Taung" by T. C. Partridge (*Nature*, 246, 75; 1973), in the section "Nickpoint Migration", the first line should read "Figure 2 shows the locations of the sites . . .".

Corrigendum

In the article "Canadian Tropical Research in the West Indies" by Finn Sander (*Nature*, 244, 201; 1973), it was wrongly stated that the Bellairs Research Institute is the only Canadian teaching and research institute in the tropics. A similar research institute, attached to the University of Montreal, and stressing studies in anthropology and sociology, functions in Martinique. The author expresses his apologies to the University of Montreal.

Reports and Publications

not included in the Monthly Books Supplement

Great Britain and Ireland

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